POLITICAL GOALS, ECONOMIC CONSTRAINTS: EXPLAINING THE MOTIVATION AND EFFECTS OF ECONOMIC SANCTIONS

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Abstract

This dissertation explores economic sanctions in an empirical political economy context. While consisting of three independent papers, it aims at providing a holistic understanding of the motivation and effects of sanctions in particular, and the interplay between economic incentives and political goals in general. My research delineates the economic constraints that policymakers encounter in the field of international relations.

Zusammenfassung

Diese Dissertation untersucht ökonomische Sanktionen im Kontext der empirischen politischen Ökonomie. Obwohl sie aus drei unabhängigen Kapiteln besteht, ist das übergeordnete, verbindende Ziel dieser Forschungsarbeit ein Gesamtverständnis der Motivation und der Effekte von Sanktionen anzubieten, getragen von der generellen Idee der Wechselwirkungen zwischen ökonomischen Anreizen und politischen Zielen. Meine Forschung zeichnet die ökonomischen Restriktionen ab, mit denen sich die politischen Entscheidungsträger im Bereich der internationalen Beziehungen auseinandersetzen.

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Chapter 1: Introduction

This dissertation studies three important questions in political economy: The targeted country voters' reaction to sanctions, the importance of trade dependencies in the decision to intervene with sanctions in a human-rights violating country and the persistent effects which trade interruption has as a result of a sanction period. I study these questions empirically in three papers, two of them spanning the whole post-1945 world and one focusing on present-day Russia. All three papers evolve around economic sanctions – what motivates them, what are their political consequences, and what are the (long-run) economic effects.

The issue of economic sanctions is a relatively neglected problem in the economics literature. The political sciences have contributed most to our understanding of sanctions. Economic sanctions are seen as an alternative to the use of force in international relations. The reasoning behind the choice of sanctions as a policy tool is that they are believed to be able to change the behavior of another country, without causing the economic and human destruction, which a war involves. If that is indeed the case, then sanctions should be recommended as a useful foreign policy instrument. If not, then policymakers should be aware of the potential sanctions pitfalls.

My research studies the deliberate or involuntary divergence between economic incentives and political goals in sanctions motivation and sanctions effects. Due to their design, sanctions impose some costs not only on the target but also on the sender. To what extent do these economic costs constrain the sanction sender in pursuing political goals?

Sanctions' main hope is to change the target's policy, either through changing the domestic political balance in the target country or through the dissatisfaction of

the target country populace with its government due to the economic suffering caused by sanctions. Is that the case? What if sanctions' economic costs induce an unexpected, unwanted reaction of unity and strengthening of the targeted regime?

There is also substantial variation in the length of sanctions. At what point in time do sanctions change persistently and potentially irreversibly the economic relations between the sanctioner and the target?

This thesis aims to understand not only the limits of political actions in the face of economic constraints but also how those economic constraints induce puzzling political realities.

In Chapter 2, *"Did sanctions help Putin?"*, I examine the political consequences of sanction imposition on elections in Russia, between 2012 and 2018. In particular, I focus on the shock induced by the Western sanctions against Russia, which were adopted in 2014. Theoretical contributions from the public choice literature on sanctions conjecture that sanction imposition may prompt the target's country citizens to "rally around the flag" by increasing their support for the regime (Kaempfer and Lowenberg, 1988, 1992). While there exists case-study- and cross-country-level evidence supporting these theoretical implications (Marinov, 2005; Takyeh and Maloney, 2011; Hirt, 2014), there has been no attempt at identifying the causal impact of sanctions on electoral outcomes.

I construct a spatially varying measure of the sanction shock for each polling station in Russia. This measure utilizes the following pieces of information: (1) the exact geographic location of a sanctioned Russian firm and (2) the exact geographic location of each of the 90000 polling stations in Russia. I geographically match polling stations to sanctioned firms in their vicinity.

In the first step of the analysis, I estimate the effect of the sanction shock on election outcomes across polling stations. I implement a difference-in-difference estimator and compare polling stations which feature a sanctioned firm in close vicinity to polling stations which are not geographically close to any sanctioned firm. I investigate the impact of sanction imposition in 2014 on the change in

Putin's vote share between 2012 and 2018. I find evidence that sanction imposition increased Putin's electoral approval by 1.5 to 3.0 percentage points, depending on the specification. This shift is sizable compared to the 13.1 percentage point overall shift in support for Putin between 2012 and 2018. Heterogeneity results suggest that these electoral responses are stronger at polling stations that show relatively low support for Putin in the 2012 presidential elections.

In the second part of the paper, I deepen the analysis by exploring alternative explanations for the increased vote share in favor of Putin. In particular, I use finance data on the sanctioned firms to measure the impact of sanctions at the firm level. I explore if voters react to the loss of local jobs induced by sanctions. A lack of punitive reaction from voters in vicinity of sanctioned firms experiencing economic losses may indicate the acceptance of the narrative of Western responsibility and "rallying around the flag".

The findings have implications for the understanding the impact of modern-day "smart" sanctions that were precisely introduced with the goal to affect only specific groups related to the ruling elite and prevent unintended consequences.

In Chapter 3, *"Strategic goods trade bias in human rights sanctions",* I study the importance of trade for the decision to impose a sanction. Economic sanctions inflict a "deadweight loss of utility" due to lost welfare benefits for all the parties involved (Pape 1997, Eaton and Engers 1999, Drezner 2003). Specifically, sanctions may disrupt trade flows that are of importance to the countries involved (Hufbauer et. al. 1997).

The literature studying the importance of trade for sanctions use does not speak with one voice. I attribute the difference in previous findings to the approach used to describe trade linkages. I claim that trade interdependence is more complex than the volume of bilateral trade employed in previous studies. This paper aims to bring clarity on the effect of trade on sanctions use by asking whether sanctions are less likely to be initiated when – inter alia – strategic economic relations between the potential sender country to the target country are stronger. In particular, I argue that human rights-concerned countries impose

sanction for human rights violations, but do so selectively. The countries that disregard human rights are sanctioned less often if they engage in trade in strategic goods, such as natural resources, armaments or high tech goods with the human rights-concerned countries. Variations in the degree to which countries that violate human rights import or export strategic goods explain the differing treatment with sanctions that result from comparable human rights violations.

Using a country-pair panel of 43 sanctioning countries and 91 human rights-violating countries between 1972 and 2005, I find strong evidence for selective human rights sanctions. Human-rights-abusing countries are less likely to be sanctioned if they import nuclear materials and armaments or export energy and nuclear products from potential sanction senders. Higher exports of chemicals and electronics as well as higher imports of chemicals increase the probability to be sanctioned by a human rights-concerned country. Being geopolitically closely aligned does not explain the impact of strategic goods trade on sanction initiation whereas trade interdependencies have explanatory power for some strategic goods. In particular, I find that strategic trade has a stronger effect on the use of sanctions for sanction senders that are more dependent on the potential target or that find it harder to substitute the trade relationship with the potential target. The logic behind these findings is that not every trade flow disruption has the same opportunity costs.

In the last Chapter 4, *"Sanctions effect persistence",* I investigate the impact of a sanction on target's trade, with both the sender and other non-sanctioning countries, during and after the sanction period.

Sanctions may disrupt the political and economic relations between the sender and the target. The reversibility of this disruption depends on the intensity of the sanction, but also on the sanction period and the availability of substitute trade partners. To the best of my knowledge, my research is novel in terms of trying to determine the persistent effect of an economic sanction on the bilateral trade relations between the parties involved. In this regard, my study adds to the well-

established results by Nitsch and Wolf (2013) and Felbermayr and Gröschl (2014) that the recovery from trade disruption is a very long process.

My results show that while sanction imposition depresses bilateral trade relations between sender and target, it does not decrease the target's total trade. A sanctioned country is able, on average, to find other trade partners. In particular, it is not the geopolitically close countries that intensify their trade relations with the target but those countries, that seem to profit from the trade disruption between sender and target – the countries that have an export mix similar to the one of the sanction sender. Moreover, trade relations between the sender and target do not pick up even after the sanction has been lifted. The sender's decision to sanction has persistent and equilibrium-changing effects on trade.

Chapter 2: Did Sanctions Help Putin?

Do sanctions strengthen the targeted regime? I analyze the 2014 imposition of Western sanctions on Russia and its impact on voting. The US and the EU introduced targeted measures against Russian entities and individuals related to Putin's regime. Using polling station-level data I investigate whether Putin gained relatively more support among those local constituencies which were geographically close to a sanctioned firm. I find a significant effect of targeted sanction imposition on the vote share in presidential elections between 2012 and 2018. Putin gained 1.54 percentage points at those polling stations that had a sanctioned firm in immediate vicinity. Targeted sanctions imposition also affected voter turnout. The effect on voting can be explained as rally-around-the-flag in the face of sanctions, as long as voters did not endure economic losses through a decline in some sanctioned firms' economic performance.

JEL-Codes: D72, P26

Keywords: sanctions, rally-around-the-flag, voting, Russia

1. Introduction

Do sanctions have an unaccounted for effect of strengthening the sanctioned regime? According to the public choice literature, sanctions can reduce the political resources of the ruling elites in the target country, thereby changing the domestic political equilibrium and bringing about a change in policy in the direction aimed by the sanction senders (Kaempfer and Lowenberg, 1988). Specifically, the regime's opposition may be encouraged by foreign sanctions and their ability to mobilize people to collective actions against the government may increase, or regime supporters may turn away from the target country rulers in

anticipation of a regime change (Kaempfer et. al., 2004). Alternatively, sanction imposition may induce the target country's citizens to reject foreign inference by increasing their support for the rulers and thereby reinforcing the sanctioned policy or behavior, a phenomenon termed "rally-around-the-flag" (Kaempfer and Lowenberg, 1992). Aware of these potential unintended consequences, sanction policymakers in the last two decades have started applying so-called "smart" or "targeted" sanctions, i.e., sanction programs which meticulously target only a country's ruler and her closest supporters (Tostensen, 2002; Drezner, 2011).

Understanding the impact of this new type of sanctions is of interest not only for sanctions policymakers and the sanctions literature but also for models of political support and state legitimacy. And for those taking the decisions on sanctions, if smart sanctions increase the popular support of a targeted government, then sanctions in general may turn out to be an obsolete, ineffective foreign policy tool.

In this paper, I empirically examine the effect of smart sanctions on the targeted country. In 2014, the EU and the US introduced sanctions against several hundreds of Russian entities and individuals. I investigate the political consequences of these sanctions on elections in Russia, between 2012 and 2018. The targeted manner of sanctioning created substantial geographical variation in direct exposure to sanctions. Russians living close to and potentially working at sanctioned firms may have experienced sanctions in a different way than the average Russian citizen. Direct exposure to local sanctioned entities may have given rise to an identity- or economic-based reaction. The local presence of sanctioned firms may have induced defiant attitudes against foreign influence and awakened or strengthened ideas of nationalism or identity from which Putin's support benefits (Pape, 1997). At the same time, sanctioned entities may have contributed to worsening local economic conditions, for which Western interference could be blamed.

This paper analyzes whether exposure to smart sanctions affects political support for the targeted regime. To do so, I assemble a panel of newly-collected polling station-level data on presidential elections and match it with geographical and financial data on sanctioned Russian firms. I then compare the change in Putin's vote share between 2012 and 2018 for the polling stations that had a sanctioned firm in close vicinity after 2014's sanctions imposition to those polling stations that did not. I find that local presence of a sanctioned firm significantly increased Putin's vote share in the 2018 presidential elections by 1.54 percentage points. Since more than 11,000 polling stations (out of over 90,000) were close to at least one sanctioned firm in the 2018 elections, the estimated effect implies over 280,000 influenced voters.

In a second part of the analysis, I show that the effect of a nearby sanctioned firm varies with local support for Putin. The effect is particularly strong at those polling stations that are the most and the least supportive of Putin. Additionally, the presence of a sanctioned firm increased voter turnout at those polling stations where Putin enjoyed highest support. The impact of sanctions on voters seems to work, at least in part, through mobilization of nonvoters in pro-Putin areas.

Pinpointing the precise mechanism that drives up Putin's support at polling stations close to sanctioned firms is challenging due to the lack of disaggregated data on voter attitudes. To confront this challenge, this paper uses firm-level data on employment at sanctioned firms between 2013 and 2017. I show that the sanctioned firm effect on voting is only present for those firms that gain additional employees over the sanctions period. The effect for sanctioned firms losing employees over the same period is negative, albeit statistically insignificant. This may be taken as an indication that an identity- or nationalism-based explanation for Putin's support is subordinate to a rational economic explanation. When sanctions affect one's livelihood and economic prosperity, Russians may be less eager to see the blame in foreign interference and to rally-around-the-flag.

My paper contributes to the literature on the domestic political impact of sanctions. One strand of this literature has found that sanctions lead to popular

mobilization against the regime in the target country and policy reversal or step-down of the regime (Kirshner, 1997; Mack and Khan, 2000; Bolks and Al-Sowayel, 2000; Marinov, 2005). These findings have been questioned by contributions that demonstrate that the impact of sanctions, especially on autocratic regimes, is probably weak (Galtung, 1967; Lektzian and Souva, 2007, Allen, 2008; Escriba-Folch and Wright, 2010). Nondemocratic regimes are able to mitigate the domestic political costs of sanctions by increasing government spending or taking repressive measures. These empirical studies typically use a cross-country or a case-study approach and are plagued by endogeneity problems, particularly in isolating the effect of sanctions on domestic politics from other concurrent dynamics or factors. I improve on these existing contributions by providing causal identification of the impact of sanctions on the target country's electoral outcomes.

My paper is also related to the small but growing literature on targeted sanctions. Dreger et al. (2015) and Tuzova and Qayub (2016) use VAR models in an attempt to estimate the impact of Western sanctions on the Russian economy. Moret et al. (2016) and Ahn and Ludema (2016) examine the change in trade flows between Russia and the rest of the world following sanction imposition. Whereas these studies give a fundamental macroeconomic perspective on how sanctions may play out across the economies involved in sanctions, they may be less well-suited to measure the impact of targeted sanctions, which are mostly affecting only specific entities or at most specific sectors of an economy. In this regard, Crozet and Hinz (2016), Haidar (2017), Ahn and Ludema (2017) and Draca et. al. (2017) take up a micro-level approach in determining the impact of sanctions on trade flows or economic performance of the sanctioned firms or sectors. Some of the findings support that smart sanctions have been able to negatively impact the performance of the entities connected to the business and political elites in Russia and Iran (Ahn and Ludema 2017; Draca et. al., 2017). Yet, Crozet and Hinz (2016) and Haidar (2017) use customs data to demonstrate that recent targeted sanctions have also had unintended consequences of deflecting trade flows, particularly in the cases of Russia and Iran. I build upon this micro-data-

based research agenda on exploration of the impact of sanctions in the following ways. First, I compile firm-level data on sanctioned firms and polling-station-level data across presidential elections in order to explore the precise working-out of Western sanctions in Russia. Second, I complement the existing contributions, which focus on real economic performance of the sanctioned entities, whereas my paper draws implications for the sanction effectiveness in terms of their political consequences. This is important because, ultimately, whether sanctions work comes down to whether or not the targeted regime changes its behavior. In this regard, the economic losses endured by the target may prove a mixed blessing – it is both possible that the regime is split or that its support is reinforced (Kaempfer et. al., 2004).

Finally, my findings may provide more insight in non-Western-centric concepts of statehood and legitimacy. If sanctions do not fracture the target government but instead increase its popular support, then it is questionable that even modern-day targeted sanctions may be able to divide the masses from the elite who is to be punished and achieve their intended goal (Freedman, 1998).

The remainder of the paper is structured as follows. Section 2 gives a brief background on Western sanctions imposition against Russia starting 2014. In section 3 I describe the data whereas section 4 presents the empirical specification, the results and robustness checks. Section 5 expands the analysis to heterogeneity effects and potential mechanisms. Section 6 concludes.

2. Sanctions background

In 2013 Ukrainian President Viktor Yanukovich refused to sign an Association Agreement with the EU, which aimed at integrating Ukraine more closely with the EU. The President supported a pro-Russian orientation. His position instigated mass protests in Ukraine and the formation of an anti-Russian and pro-European movement. An Ukrainian government-led intervention against the protesters led to several deadly incidents. The conflict culminated with President Yanukovich fleeing the country and Russia invading the Crimean peninsula in March 2014. A referendum held at Crimea shortly thereafter affirmed the peninsula's decision to join the Russian Federation. As these proceedings were not abiding by international law, the EU and the US imposed sanctions on Russian politicians and against specific economic entities in Crimea. These actions were accompanied by pro-Russian and pro-Ukranian protests across Ukraine and a rising polarization of the country. The conflict continued escalating in the eastern parts of Ukraine – Donetsk and Lugansk – and Russia was accused of supporting pro-Russian militant activists in those regions. This led to further sanctions adoption by the US and the EU against Russia. Russia answered with countersanctions against the sanctioning countries.

Sanction specifics – The US issued four Executive Orders[1] between March and December 2014, which authorize US government institutions to impose and regulate sanctions against Russia. US sanctions consist of two non-exclusive categories. The first category, SSI (Sectoral Sanctions Identification) sanctions, are sanctions aimed at entities in the Russian financial, energy and defense sectors. The following restrictions apply for these entities: US citizens are prohibited from transacting or issuing debt of a maturity of more than 30 days or acquiring new assets of the sanctioned entities. Additionally, the transaction of certain technologies and services related to deep-water, offshore or shale oil activity is also prohibited. The second sanctions category, SDN (Specially

[1] Execute Order 13660 (March 6, 2014), Execute Order 13661 (March 16, 2014), Execute Order 13662 (March 20, 2014), and Execute Order 13685 (December 19, 2014).

Designated Nationals and Blocked Persons) sanctions, deals with individuals and entities which are to be fully blocked from any economic activity with the US.

The EU sanctions policy is quite similar to the US one and is set out in several EU Council Regulations.[2] The EU also maintains two broad sanction categories. The first one, Sectoral Sanctions List, prohibits EU citizens to transact in debt or equity of a maturity exceeding 30 days with entities on the list. It also bans EU exports of deep-water, offshore or shale oil related technologies and services. The other category, Restricted Measures List, prohibit the issuance of visas and freezes the assets for all individuals featured on the list as well as prohibits any economic activities with entities and individuals on the list.

3. Data and descriptive statistics

3.1 Data and construction of the dataset

3.1.1 Elections data

Elections data comes from the Central Election Commission of the Russian Federation and was webscraped from the website of the Commission[3], where it is made available at the polling station level. Address data of the polling stations for the 2018 presidential elections was also available from the Election Commission. Polling stations are formed for a period of five years. While most of them retain their identification number and localization across elections, some do not. In order to account for potential changes in polling stations between the 2012 and 2018 elections, I use data on the addresses of the polling stations in 2012 collected by GIS-Lab Russia. This non-governmental organization is a society of specialists in geographic information systems, which runs specific geographic and remote sensing projects and makes their data available online.[4] I then geocode the polling station addresses for the 2012 and 2018 presidential elections using Yandex Maps and Google Maps APIs. To match polling stations

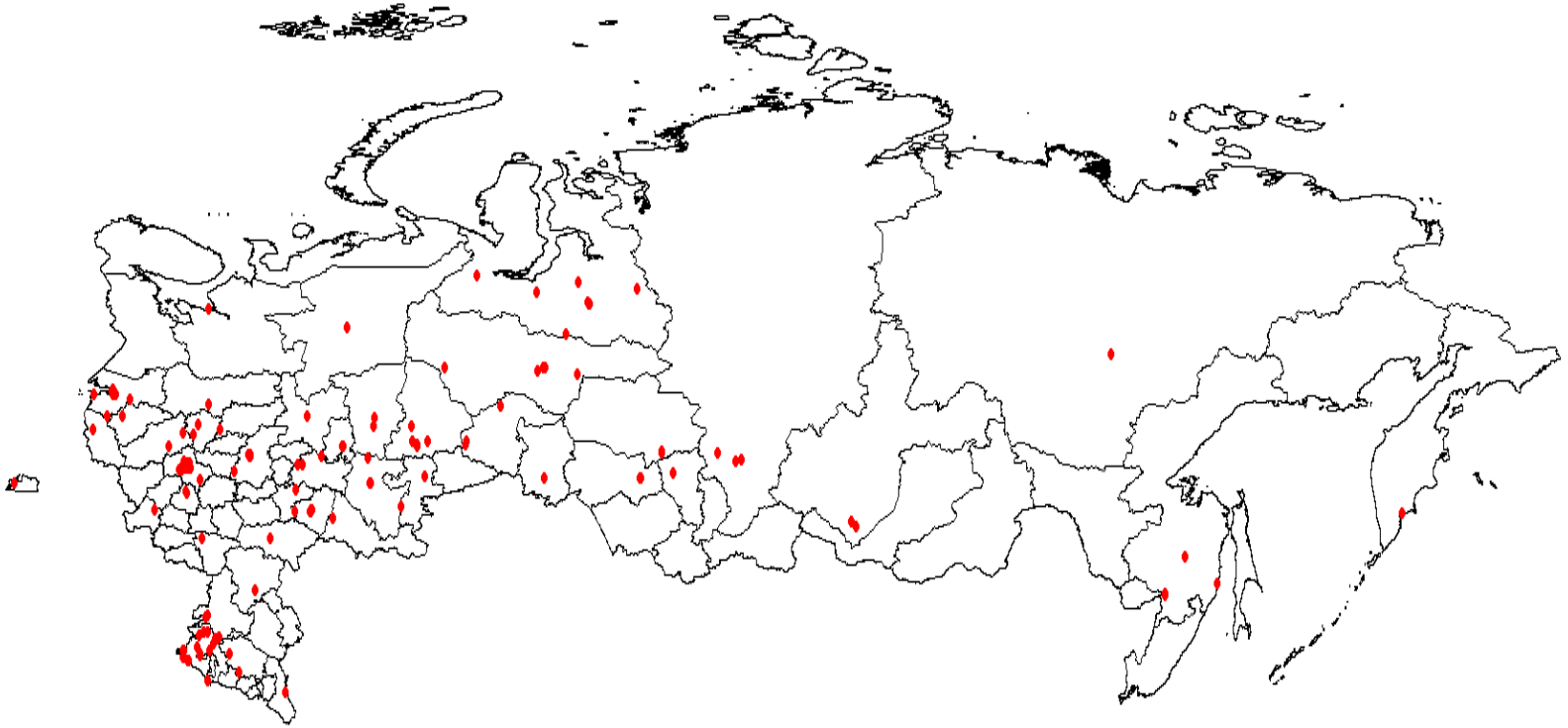
[2] EU Council Regulations 269/2014, 284/2014, 433/2014, 833/2014 and 960/2014.
[3] www.cikrf.ru
[4] gis-lab.info

across elections, I use Stata's *geodist* routine (Picard, 2010), which calculates straight-line distance between two geographical coordinates. Roughly 5,000 polling stations from the 2018 elections remain unmatched (out of 90,000). Additionally, of those matched, 3,671 of the polling stations matched feature a matching distance of more than 10 km. I drop these as unlikely or erroneous matches. I am then left with 79,922 matched polling stations, with an average (median) matched distance between 2012 and 2018 polling stations equal to 0.8 km (0.3 km) and the 95th percentile corresponding to 3.6 km distance between matched polling stations. The election data, consisting of votes for the different candidates, number of eligible voters registered and the total ballots cast is kept at the polling station level.

3.1.2 Sanctions data

I identify 361 distinct firms located in Russia from the US and EU sanctions lists. Firms are listed with their addresses which I cross-check with firm-level data provided by Bureau van Dijk's AMADEUS database. This database provider collects and standardizes financial and ownership data on firms located in Europe. The firms' locations are geocoded using Yandex Maps and Google Maps API so that I can match them to polling stations across Russia. The firms are located all across Russia (Figure 1), however with a high prevalence in Moscow and St. Petersburg – 218 firms are located there, i.e. 60% of all sanctioned firms. Overall, sanctioned firms are located in cities of sizable population – another 29 sanctioned firms are located in cities with population of at least 1,000,000; 69 firms are located in mid-sized cities of population between 100,000 and 1,000,000 and the remaining 45 sanctioned firms operate from locations with population under 100,000 (Table 2.1).

FIGURE 2.1: SPATIAL DISTRIBUTION OF SANCTIONED FIRMS



Note: This map shows the distribution of sanctioned firms across Russia. The size of the marker is not proportionate to the number of firms at a given location.

TABLE 2.1: POPULATION SIZE BY SANCTIONED FIRM LOCALITY

Population at sanctioned firm location	Frequency	%	Cum. %
Population > 2,000,000	218	60.39	60.39
1,000,000 < Population <= 2,000,000	29	8.03	68.42
100,000 < Population <= 1,000,000	69	19.11	87.53
Population < 100,000	45	12.47	100.00
Total	361	100.00	

Most of the sanctioned firms are small to mid-sized firms, with three-quarters of the firms with less than 1,000 employees in 2013 (Table 2.2).

TABLE 2.2: SIZE OF SANCTIONED FIRMS

Number of employees at sanctioned firm in 2013	Frequency	%	Cum %
Employees < 50	53	26.11	26.11
50 < Employees <= 1,000	102	50.25	76.35
1,000 < Employees <= 10,000	46	22.66	99.01
Employees > 10,000	2	0.99	100.00
Total	203	100.00	

The distribution of sanctioned firm size across the largest (Moscow and St. Petersburg) and the smallest cities is similar, with a prevalence of small and mid-sized sanctioned companies, whereas most of the largest sanctioned firms are located in cities of population between 100,000 and 2,000,000 (Table 2.3).

TABLE 2.3: SANCTIONED FIRM SIZE AND FIRM LOCATION POPULATION

	Employees < 50	50 < Employees <= 1,000	1,000 < Employees <= 10,000	Employees > 10,000	Total
Population > 2,000,000	30.83	54.14	15.04	0.00	100.00
1,000,000 < Population <= 2,000,000	6.25	37.50	50.00	6.25	100.00
100,000 < Population <= 1,000,000	13.79	31.03	51.72	3.45	100.00
Population < 100,000	28.00	60.00	12.00	0.00	100.00
Total	26.11	50.25	22.66	0.99	100.00

To assess the effect of a sanctioned firm on local voters' support for Putin, I would ideally interview local voters on their political attitudes, prior and after the sanctioning of local firms. Unfortunately, this kind of information at such level of geographic detail is not available. I therefore use spatial vicinity of a local constituency to a sanctioned firm as a proxy for exposure. From the data described above, I construct several treatment variables. The main one identifies all polling stations which have one (or more) sanctioned firm within a radius of 10 km. There are 11,068 polling stations that were treated with at least one nearby sanctioned firm. In robustness checks I vary this distance to include only very close (within 3 km) or also more faraway-located (within 60 km) sanctioned firms. In addition, I construct a treatment variable which accounts for the potential economic intensity of treatment. In particular, I draw a radius of 10 km around each polling station and sum the number of employees at sanctioned firms within this distance. I then relate this number to the total population in the subregion in which the particular polling station is located. I thus have a measure giving me the ratio of local population employed at sanctioned firms.

3.1.3 Control variables

The polling stations are organized in electoral districts, which match Russian subregions. The subregion is the lowest administrative level at which data is collected by Rosstat, the Russian Federal State Statistics Service. I collect demographic data from Rosstat's website[5] for all 2,351 subregions, which in most cases is available for the period 2009-2017. The variables that provide enough coverage are total population, as well as population shares according to age, gender, social benefits recipients and urbanization. To account for potential economic confounders that may influence both the location of a sanctioned firm and political support for Putin, I also collect several economic performance controls. These include goods and services produced, state investment in fixed assets, and average wage, all at the subregional level, provided by Rosstat.

3.2 Summary statistics

Table 2.4 presents summary statistics of the election, demographic and economic data. I compare polling stations that featured a sanctioned firm within 10 km distance (column(2)) to polling stations that did not (column (1)). Those polling stations with a sanctioned firm are located in economically stronger subregions: average wages are higher, as is state investment per capita and the value of goods and services produced. The subregions in which the sanctioned polling stations are located are more than six times larger and almost exclusively urban (96.18% urban population).

[5] www.gks.ru

TABLE 2.4: SUMMARY STATISTICS

	(1) No sanctioned firm within 10km	(2) Sanctioned firm within 10km	(3) Total
Election variables (polling station level)			
Putin's vote share (%) in 2012	67.42	55.49	65.17
	(11.78)	(9.55)	(12.31)
Putin's vote share (%) in 2018	77.82	73.68	77.04
	(8.84)	(5.42)	(8.46)
Turnout (%) in 2012	69.18	62.19	67.86
	(13.42)	(9.16)	(13.02)
Turnout (%) in 2018	72.34	63.27	70.63
	(14.21)	(9.30)	(13.88)
Demographic variables in 2017 (subregion level)			
Population, 2017	97,788	623,332	196,774
	(187,189)	(484,454)	(338,918)
Male (%), 2017	47.49	45.20	47.06
	(2.03)	(1.08)	(2.09)
Elderly (%), 2017	26.38	24.16	25.96
	(4.45)	(2.61)	(4.26)
Social benefit recipients (%), 2017	25.91	22.99	25.36
	(9.30)	(5.67)	(8.81)
Rural population (%), 2017	53.65	3.82	44.27
	(38.31)	(15.61)	(40.21)
Economic variables in 2017 (subregion level)			
Average wage (in RUB), 2017	26,825	48,470	30,901
	(9,362)	(18,949)	(14,505)
State investment in fixed assets (RUB per capita), 2017	1,105.08	24,157.39	5,446.98
	(3,433.80)	(135,826.18)	(59,710.75)
Goods and services produced (in logs), 2017	22.35	25.46	22.93
	(2.13)	(1.70)	(2.39)
Demographic variables, change from 2011 to 2017 (subregion level)			
Population, change, 2017-2011	806	29,530	6,216
	(9,458)	(34,369)	(20,523)
Male, % points change, 2017-2011	0.54	0.04	0.44
	(0.99)	(0.70)	(0.96)
Elderly, % points change, 2017-2011	2.69	1.17	2.41

	(1.18)	(1.28)	(1.34)
Social benefit recipients, % points change, 2017-2011	-1.68 (7.94)	-1.39 (4.47)	-1.62 (7.41)
Rural population, % points change, 2017-2011	-0.04 (4.65)	-0.13 (0.66)	-0.06 (4.20)
Economic variables, change from 2011 to 2017 (subregion level)			
Average wage (in RUB), change, 2017-2011	4,895 (2,573)	8,765 (6,910)	5,624 (4,081)
State investment in fixed assets (RUB per capita), change, 2017-2011	-83.66 (4,171.94)	9,379.55 (67,604.71)	1,698.74 (29,809.18)
Goods and services produced (in logs), change, 2017-2011	0.19 (0.43)	0.27 (0.54)	0.21 (0.45)
Observations (number of polling stations)	47,695	11,068	58,763

Notes: Standard deviations in parentheses. The subset "No sanctioned firm within 10km" is formed by the polling stations which have at least one sanctioned firm within a 10 km distance. The subset "Sanctioned firm within 10km" consists of those polling stations which do not have any sanctioned firms within a 10 km distance. Observations are unweighted.

The polling stations that have a nearby sanctioned firm increased their support for Putin by 18.19 percentage points (from 55.49% to 73.68%). At those polling stations where there are no neighboring sanctioned firms, Putin's vote share rose by 10.40 percentage points (from 67.42% to 77.82%). These numbers are already suggestive of the vote-increasing effect a sanctioned firm may have had on local constituencies in the presidential elections in 2018. Yet, this implication does not consider the difference in the number of eligible voters by polling station, nor does it account for the differing voting trends across demographic and economic characteristics. In my subsequent estimation I account for all these factors.

The sample consists of 58,863 polling stations, out of approximately 90,000. These polling stations are spread across 1709 Russian subregions, out of a total of 2351 subregions.

4. Empirical specification and results

I estimate the effect of having a sanctioned firm in close vicinity on voting behavior using a difference-in-difference approach. The treatment received by voters at a given polling station is the availability of a sanctioned firm within 10 km distance from the polling station. Measuring the effect of sanctioned firms involves comparing the changes in voting behavior of the electorate at polling stations where a firm is sanctioned after 2014 relative to the differences in voting behavior at those polling stations where no firm is sanctioned after 2014.

The difference-in-difference approach guards against certain threats to identification of the treatment effect. Looking at the differences in vote share across time assures that time-invariant characteristics of the polling station or the voters cannot be the reason for the change in voting behavior. Moreover, any time-varying effects are also controlled for by comparing the change in vote share between treated and untreated polling stations. Hence, the comparison of the change in the treatment group relative to the control group allows me to distill the effect of the treatment. In order to be able to attribute the difference in changes between the two groups as the effect of sanctioned firms, the treatment has to be unconditionally or conditionally (based on controls) exogenous. Specifically, the US and the EU should not have sanctioned firms that are close to polling stations where voters are more likely to increase their support for Putin, even if there are no sanctioned firms nearby. Since the assignment of polling stations in treated and unterated has not been made randomly, I explore the determinants of selection and pinpoint the variables predicting the occurrence of a sanctioned firm in 2018.

The firms that have been sanctioned by the US and EU starting 2014 may be located in areas that feature strong support for Putin. Indeed, the proclaimed aim of the US and the EU has been to use sanctions to hurt Russian political and business elites, who support the Russian annexation of Crimea and the Russian regime. If the sanctioned firms are situated next to polling stations that were

becoming more pro-Putin between 2012 and 2018, then the estimated effect of sanctioned firms in vicinity would be probably capturing that political trend.

The treatment variable, $T10_{ij,2018}$ is a dummy variables, measured at the polling stations level and equal to one if there is at least one sanctioned firm within 10 km of the polling station; and equal to zero otherwise. To examine the determinants of the assignment to treatment, I estimate the following linear probability model:

$$T10_{ij,2018} = \alpha + \beta_1 share_{ij,2012}^{Putin} + \beta_2 turnout_{ij,2012}^{Pres.elec.} + \boldsymbol{B}_{2017}\boldsymbol{X}_{j,2017} + \boldsymbol{B}_{2017-2011}\boldsymbol{X}_{j,2017-2011} + \varepsilon_{ij} \quad (2.1)$$

Of particular interest are the two political variables at the polling station level – Putin's vote share in 2012, $share_{ij,2012}^{Putin}$, and voter turnout, equal to the ratio of votes cast to eligible voters at polling station *i* in subregion *j* (in percentage), $turnout_{ij,2012}^{Pres.elec.}$. Further determinants of the treatment are demographic and economic variables measured at the subregion level. These are the set of controls for 2017 - $\boldsymbol{X}_{j,2017}$ and the set of changes in controls between 2011 and 2017, $\boldsymbol{X}_{2017-2011}$. To account for the different number of eligible voters across polling stations I weigh the observations by number of eligible voters at a polling station in 2012.[6] I cluster the standard errors at the subregion level.

The results of this estimation are presented in Table 2.5. In column (1), I first estimate the linear probability model without controls and fixed effects. The presence of a sanctioned firm is positively and statistically significantly related to turnout and negatively and statistically significantly related to Putin's vote share in 2012. Adding demographic characteristics to the estimation (column (2))

[6] The results remain qualitatively similar when I weigh the observations by the number of eligible voters in 2018 or by the number of total votes cast at a polling station in 2012 or 2018.

reduces the magnitude of those relationships as does the addition of economic controls (column (3)). Next, I include subregion fixed effects (column (4)). This specification accounts for the determinants of within-subregion location of sanctioned firms, controlling at the same time for demographic and economic characteristics. Although the coefficients on the political variables are still statistically significant, the addition of the geographic fixed effects further lowers both political variables coefficients to magnitudes which are close to zero. Moreover, polling stations with higher pro-Putin vote share are less likely to feature a nearby sanctioned firm – the estimated coefficient is -.001. Running an F-test for statistical significance of all demographic or all economic controls shows that I cannot reject the hypothesis that these control variables are zero (F-test = 0.95 / 0.94). The presence of a sanctioned firm is therefore uncorrelated with demographic and economic variables once geographic heterogeneity is accounted for and negatively related to voters' support for Putin. While sanctioned firms tend to be present in more urban and economically vibrant areas (see Table 2.1), the assignment to polling stations within a subregion is plausibly random. In the last column (5) I examine the impact of voting trends prior treatment on the predictability of sanctioned firm occurrence. The vote share change for United Russia[7] between the two presidential elections in 2008 and 2012 is not statistically significant.

[7] Dmitry Medvedev and not Vladimir Putin was the party's candidate in 2008

Table 2.5: DETERMINANTS OF SANCTIONED FIRMS LOCATION

Dependent variable: Occurrence of a sanctioned firm in 2018					
	(1)	(2)	(3)	(4)	(5)
Putin's vote share (%) in 2012	-0.0173***	-0.0087***	-0.0019**	-0.0010***	-0.0010***
	(0.0010)	(0.0010)	(0.0009)	(0.0002)	(0.0002)
Turnout (%) in 2012	0.0027***	0.0020***	0.0018***	0.0004***	0.0004***
	(0.0009)	(0.0007)	(0.0006)	(0.0002)	(0.0002)
United Russia's vote share change (%), 2008-2012					0.0000
					(0.0001)
F-test: Demographic controls = 0		71.33	45.60	0.95	0.95
F-test: Economic controls = 0			40.57	0.94	0.94
Demographic controls	no	yes	yes	yes	yes
Economic controls	no	no	yes	yes	yes
Subregion FE	no	no	no	yes	yes
R-squared	0.15	0.44	0.60	0.93	0.93
N	58763	58763	58763	58763	58763

Notes: Linear probability model with unit of observation polling station. The dependent variable is a binary variable that equals one if a sanctioned firm was within 10 km distance of the polling station in 2018. The turnout measure is the ratio of total votes cast in 2012 to number of eligible voters at the polling station in 2012. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. The F-test is a joint test of the hypothesis that the demographic and/or economic controls are jointly equal to zero. Robust standard errors clustered by subregion in parentheses. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.1 Main result

I compare polling stations featuring at least one sanctioned firm within 10 km distance ($T10_{ij,2018}$=1) to polling stations with no sanctioned firms within 10 km distance ($T10_{ij,2018}$=0). I am interested in examining the impact of sanctioning a nearby firm on the change in Putin's vote share at the polling station level between 2012 and 2018. Sanctions were adopted starting 2014. My specification is as follows:

$$share_{ij,2018}^{Putin} - share_{ij,2012}^{Putin} = \alpha + \beta_1 T10_{ij,2018} + \boldsymbol{B}_{2017}\boldsymbol{X}_{j,2017} + \boldsymbol{B}_{2017-2011}\boldsymbol{X}_{j,2017-2011} + \varepsilon_{ij} \quad (2.2)$$

Similar to the specification examining the determinants of sanctioned firm availability, I again control for subregion-level demographic and economic characteristics in levels ($\boldsymbol{X}_{j,2017}$) and in changes ($\boldsymbol{X}_{j,2017-2011}$). I weight the observations by the number of eligible voters at the polling station level in 2012 and cluster the standard errors at the subregion level.

Table 2.6 exhibits the estimation results. Absent of controls or fixed effects, the difference-in-difference estimation shows that on average, the overall support for Putin in my sample increased by over 12% ($\hat{\alpha} = 12.006$). Relative to this overall shift, polling stations with a nearby sanctioned firm increased their vote share for Putin by 6.72% relative to polling stations without a nearby sanctioned firm, a statistically significant result. Including demographic controls in column (2) almost halves the magnitude of the estimate of the sanctioned firm presence to 3.99%, but it remains statistically significant. In column (3), I add economic controls which further lowers the magnitude of the sanctioned firm effect, leaving it statistically significant. Finally, in the benchmark specification in column (4), I include subregion fixed effects, in addition to all control variables.

This specification accounts for unobservable trends in voting behavior common to a subregion that may be correlated with the presence of a sanctioned firm. The effect of a sanctioned firm on voting is identified by comparing neighboring polling stations with and without sanctioned firms, within the same subregion. The estimated effect is more precise than in the other specifications (columns (1)-(3)) and is positive, statistically significant and sizable, at 1.54%.

Table 2.6: THE EFFECT OF SANCTIONED FIRMS ON 2012-2018 PUTIN'S VOTE SHARE CHANGE

Dependent variable: Putin's vote share change between 2018 and 2012 presidential elections				
	(1)	(2)	(3)	(4)
Sanctioned firm within 10 km	6.717***	3.993***	1.499**	1.543***
	(0.428)	(0.549)	(0.587)	(0.383)
Constant	12.006***	54.683***	36.744***	-9.820
	(0.264)	(7.003)	(8.086)	(53.048)
Demographic controls	no	yes	yes	yes
Economic controls	no	no	yes	yes
Subregion FE	no	no	no	yes
R-squared	0.107	0.197	0.225	0.568
N	58763	58763	58763	58763

Notes: The unit of observation is polling station. The dependent variable is (Putin's vote share for the 2018 presidential election) - (Putin's vote share for the 2012 presidential election). The variable "Sanctioned firm within 10 km" is a binary variable that equals one if there is a sanctioned firm within 10 km distance from the polling station. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2 Robustness

In the following, I test the robustness of the above results to alternative definitions of the treatment variable as well as to placebo treatments.

Up to now, the definition of the treatment variable - exposure to a sanctioned firm – was fixed to a radius of ten kilometers around the polling station. This choice has been guided by the average commuting distance from one's home to work. Yet, my sample features firms in very heterogeneous settings – from the multimillion inhabitant cities of Moscow and St. Petersburg to rural settlements in the Northeast of Russia. The median distance travelled to work in the Moscow region was 30 kilometers (with an average of 50 km, Shitov & Shitova, 2017) and this distance declines with smaller localities (SuperJob, 2015) to around 6 km in rural settlements (Yandex, 2016).

In order to check if my results hold against different specifications of the exposure, I now vary the definition of treatment down to 3 km and up to 60 km distance from a polling station. The results are presented in Table 2.7, columns (1) and (2). The positive effect of vicinity to a sanctioned firm on Putin's vote share remains about the same for a distance of 60 km ($\widehat{\beta_1} = 1.586$) while it decreases for the smaller distance of 3 km ($\widehat{\beta_1} = 0.467$). One explanation for the strong effect at even higher distances follows from the structure of the sample of sanctioned firms. About 60 % of the firms that have been sanctioned in 2014 are located in Moscow or St. Petersburg. These are cities that attract commuters from afar while also featuring denser presence of sanctioned firms than any other locations in the sample. The effect of the existence of a sanctioned firm might have thus spilled over to more distant neighboring areas. Finally, it is reassuring to see that the main result remains valid and statistically significant with the very tight definition of exposure to treatment of 3 km (column (2)).

TABLE 2.7: ROBUSTNESS

Dependent variable: Putin's vote chare change between 2018 and 2012 presidential elections				
	(1)	(2)	(3)	(4)
Sanctioned firm within 60 km	1.586***			
	(0.520)			
Sanctioned firm within 3 km		0.467*		
		(0.284)		
Share of subregion population (%), working at sanctioned firm(s) within 10 km in 2012			0.080*	
			(0.047)	
Sanctioned firm within 10 km				1.640***
				(0.380)
Demographic controls	yes	yes	yes	yes
Economic controls	yes	yes	yes	yes
Subregion FE	yes	yes	yes	yes
Unweighted	no	no	no	yes
R-squared	0.568	0.567	0.567	0.509
N	58763	58763	58763	58763

Notes: The unit of observation is polling station. The dependent variable is (Putin's vote share for the 2018 presidential election) - (Putin's vote share for the 2012 presidential election). The variables "Sanctioned firm within 60 km" and "Sanctioned firm within 3 km" are binary variables that equal one if a sanctioned firm is within 60 km or 3 km distance from a polling station, respectively. The variable "Share of subregion population (%), working at sanctioned firm(s) within 10 km in 2012" measures the percentage share of the population of the subregion, to which the polling station belongs to, that works at sanctioned firms located in 10 km vicinity of the specific polling station in 2012. Data on employment at sanctioned firms was put together from Bureau van Dijk's AMADEUS database. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Second, I also consider the number of workers employed at all sanctioned firms within 10 km of a given polling station, relative to the total population at the

subregion level. Because the sanctioned firms vary in size, the number of employees directly affected by a sanction also varies substantially (mean employment at a sanctioned firm in 2012 = 159; median = 939). This variation in direct exposure suggests variation in treatment effects. Larger firms may imply stronger effects because they reach a wider part of the constituency through their employees and their families. Larger firms may also attract more public attention and media coverage, through which the awareness about the sanctioning of a specific firm may have been carried to a wider audience. The results from a specification with the percentage of subregion population employed at a sanctioned firm is shown in column (3) of Table 2.4. The effect of the treatment is positive, albeit less precisely estimated than the effect from my benchmark regression, which uses a simple dummy.

Finally, in column (4) I show that differences in the number of eligible voters across polling stations are not driving the results. The estimated effect of the occurrence of a sanctioned firm within 10 km of a polling station on Putin's vote share remains about the same ($\widehat{\beta_1} = 1.640$) when running an unweighted regression.

Next, I explore the possibility that unobservable confounding variables are determining both the location of sanctioned firms and the voting behavior of the electorate. Specifically, I design a placebo test and test whether the availability of a sanctioned firm within 10 km of a polling station in 2018 predicts changes in voting behavior between 2008 and 2012. There should be no effect as there were no sanctioned firms in Russia prior to 2014. Table 2.8 reveals that while the occurrence of a sanctioned firm in 2018 has a predictive power for voting behavior change between the 2008 and 2012 presidential elections, the effect is negative. This implies that there might be some unobservable features that are related both to the location of a sanctioned firm and the tendency of voters to support Putin; however, these omitted characteristics seem to be working against finding a positive effect between 2012 and 2018, as the sign of the estimated relationship is negative ($\widehat{\beta_1} = -1.579$).

TABLE 2.8: PLACEBO REGRESSION

Dependent variable: United Russia's vote share change between 2012 and 2008 presidential elections	
Sanctioned firm within 10 km	-1.579***
	(0.510)
Demographic controls	yes
Economic controls	yes
Subregion FE	yes
R-squared	0.538
N	58762

Notes: The unit of observation is polling station. The dependent variable is (United Russia's vote share for the 2012 presidential election) - (United Russia's vote share for the 2008 presidential election). The variable "Sanctioned firm within 10 km" is a binary variable that equals one if a sanctioned firm is within 10 km distance from the polling station. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The negative effect can also be explained by the unavailability of address information on the location of the 2008 polling stations. While many of the polling stations' locations and numbering are kept across elections, some polling stations do change their location, some are closed down and others are opened at new places. Thus, the 2008 polling stations have been matched to the rest of the elections data only based on their subregion information and polling station number. This method is prone to errors and may account for the negative effect that I find. To address this challenge, I aggregate the data to the next administrative level – subregion – and redo the placebo analysis. The results in Table A2.1 demonstrate that the presence of a sanctioned firm within the boundaries of a subregion in 2018 does not influence voting behavior between 2008 and 2012.

4.3 Magnitude of the effect

My benchmark specification indicates that the presence of a sanctioned firm within 10 km distance of a polling station had a significant impact on Putin's vote share in the 2018 presidential elections. The benchmark estimate from Table 2.5, column (4) implies a confidence interval of (0.790; 2.294). In what follows, I offer an interpretation of the magnitude of the estimated effects.

The average shift in favor of Putin in my sample is equal to 12.006 percentage points, with a standard deviation of 9.706. The impact of a sanctioned firm is equal to about one-sixth of the standard deviation, a reasonably sizable effect.

Additionally, I estimate the likely number of voters that shifted their votes to support Putin. By 2018, there were 11,068 polling stations which featured at least one sanctioned firm within a 10 km distance. A total of 18,615,116 votes were cast at those particular polling stations. Abstracting from voter turnout changes, these numbers imply that $0.01543 * 18{,}615{,}116 \approx 287{,}231$ voters shifted their votes from other candidates to Putin.

5. Heterogeneous effects and potential mechanisms

I have established that the presence of a sanctioned firm close to a polling station increases the electoral support for Putin. In this section, I analyze the potential mechanisms driving this effect. I start with an exploration of the heterogeneous effects of the treatment depending on support for Putin in the 2018 elections and across different geographic areas. I then look at the impact of sanctioned firms on mobilization of voters. Lastly, I exploit the rich firm-level information on the sanctioned firms to examine the most likely case for the treatment effect.

5.1 Heterogeneous effects

I examine how the sanctioned firm effect interacts with the political positions of local constituencies. In order to do so, I split the polling stations into thirds, depending on their support for Putin in the 2018 presidential elections. I then interact the treatment dummy for sanctioned firm within 10 km with the bottom and the top third of pro-Putin-voting polling stations. Table 2.9 reports the results from this estimation. Both interaction terms are positive and statistically significant - the impact of a sanctioned firm is higher not only in those communities that are most supportive of Putin but also across those polling stations where Putin enjoys the least support. These results imply that the imposition of a sanction on a local firm not only increases the support for the regime among supporters but also convinces previous opponents to vote pro-Putin.

Table 2.9: INTERACTIONS

Dependent variable: Putin's vote share change between 2018 and 2012 presidential elections	
Sanctioned firm within 10km	1.072***
	(0.374)
Most supportive of Putin polling station	4.659***
	(0.217)
Sanctioned firm within 10km # Most supportive of Putin polling station	1.173**
	(0.497)
Least supportive of Putin polling station	-3.622***
	(0.161)
Sanctioned firm within 10km # Least supportive of Putin polling station	1.173***
	(0.277)
Demographic controls	yes
Economic controls	yes
Subregion FE	yes
R-squared	0.614
N	58763

Notes: The unit of observation is polling station. The dependent variable is (Putin's vote share for the 2018 presidential election) - (Putin's vote share for the 2012 presidential election). The variable "Sanctioned firm within 10 km" is a binary variable that equals one if a sanctioned firm is within 10 km distance from the polling station. The dummy variables "Most supportive of Putin polling station" and "Least supportive of Putin polling station" are constructed by dividing the 58763 polling station observations into thirds based on Putin's vote share in the 2018 presidential elections. The variable "Most supportive of Putin polling station" indicates a polling station in the top third. The variable "Least supportive of Putin polling station" indicates a polling station in the bottom third. The omitted category indicates the middle third.
The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, I test how locality characteristics influence the effect of a sanctioned firm on voting behavior. Most of the sanctioned firms are situated in Moscow and St. Petersburg (60% of all sanctioned firms) and these two cities are quite distinct in terms of political trends from the rest of Russia. For example, the two megacities show the highest levels of support for oppositional parties (Dmitriev and

Treisman, 2012; Lankina and Voznaya, 2015). To check whether there are any differences in the impact of the availability of a nearby sanctioned firm on voting behavior between Moscow and St. Petersburg and the rest of Russia, I subset the sample along these two geographic groups. The results, which are reported in Table 2.10, show that the main result of this paper is indeed driven by voting behavior shift outside the two largest Russian cities. While the direction, strength and significance of the sanctioned firm presence coefficient remains about the same for the sample subset without Moscow and St. Petersburg, it becomes insignificant when I run the regression on the two cities only. The political divide between the urban and progressive Moscow and St. Petersburg is upheld also in the ability of a sanction imposition to sway voters' behavior.

Table 2.10: GEOGRAPHICAL HETEROGENEITY

Dependent variable: Putin's vote share change between 2018 and 2012 presidential elections		
	No Moscow and St. Petersburg	Moscow and St. Petersburg only
	(1)	(2)
Sanctioned firm within 10 km	1.637***	0.540
	(0.416)	(0.468)
Demographic controls	yes	yes
Economic controls	yes	yes
Subregion FE	yes	yes
R-squared	0.536	0.306
N	54537	4226

Notes: The unit of observation is polling station. The subset "No Moscow and St. Petersburg" is formed by the polling stations in all regions but Moscow and St. Petersburg. The subset "Moscow and St. Petersburg only" is the complementary subset. The dependent variable is (Putin's vote share for the 2018 presidential election) - (Putin's vote share for the 2012 presidential election). The variable "Sanctioned firm within 10 km" is a binary variable that equals one if a sanctioned firm is within 10 km distance from the polling station. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.2 Voter mobilization or vote swings?

Up until now, I have shown that sanctioned firms led to rise in support for Putin. However, this observation alone does not explain how Putin's increased success came about. In what follows, I examine whether the approval came from other parties' voters switching to United Russia (the party Putin is affiliated with), or from new voters that were attracted to cast their vote following the sanction imposition. It is possible that business and political elites who backed the regime and who were among the owners or managers of the sanctioned firms tried to mobilize local nonvoters to vote in favor of Putin. Indeed, there were several

reports of "corporate mobilization" attempts across Russian state-owned enterprises, which were supposedly charged with the task to get out the votes.[8] To test whether the significant impact of sanctioned firms comes from voter mobilization or voter switches, I employ the following voter turnout specification:

$$turnout_{ij,2018}^{Pres.elec.} - turnout_{ij,2012}^{Pres.elec.} = \alpha + \beta_1 T10_{ij,2018} + \beta_2 (elig.voters_{ij,2018}^{Pres.elec.} - elig.voters_{ij,2012}^{Pres.elec.}) + \boldsymbol{B}_{2017}\boldsymbol{X}_{j,2017} + \boldsymbol{B}_{2017-2011}\boldsymbol{X}_{j,2017-2011} + \varepsilon_{ij} \quad (2.3)$$

where $turnout_{ij,t}^{Pres.elec.}$ is the percentage total votes at polling station *i*, in subregion *j*, in year $t \in \{2012, 2018\}$. I control for the change in the number of eligible voters $(elig.voters_{ij,2018}^{Pres.elec.} - elig.voters_{ij,2012}^{Pres.elec.})$, as well as for the usual economic and demographic characteristics of subregions.

Comparing polling stations with and without a sanctioned firm within 10 km distance suggests that these voter mobilization groups may have been successful in attracting new voters. Table 2.11 shows that at polling stations with a sanctioned firm, voter turnout increased, but the estimated effect is not statistically significant. When I disaggregate the effect of a sanctioned firm according to the level of support for Putin at the polling station, I find evidence that the turnout effect is quite strong and statistically significant at those polling stations that are most in favor of Putin. Combining this finding with the evidence

[8] See newspaper reports from Kommersant (На выборах задействуют корпоративный ресурс, February, 26, 2017, https://www.kommersant.ru/doc/3227902), Vedomosti (Кремль начал мониторинг экономических событий, влияющих на региональные настроения, February 27, 2017, https://www.vedomosti.ru/politics/articles/2017/02/27/679036-kreml-monitoring) and Washington Post (Yes, the Kremlin is worried — about Russia's own presidential elections, December 6, 2017, https://www.washingtonpost.com/news/monkey-cage/wp/2017/12/06/yes-the-kremlin-is-worried-about-russias-own-presidential-elections/?noredirect=on&utm_term=.baa567e429b2)

from Table 2.9 implies that sanction imposition urged nonvoters at pro-Putin polling stations to vote for Putin.

Table 2.11: TURNOUT

Dependent variable: Voter turnout		
	(1)	(2)
Sanctioned firm within 10 km	1.054	
	(1.032)	
Change in eligible voters (%), 2012-2018	0.929***	0.930***
	(0.012)	(0.012)
Sanctioned firm within 10km		0.705
		(1.070)
Most supportive of Putin polling station		7.703***
		(0.487)
Sanctioned firm within 10km # Most supportive of Putin polling station		2.685**
		(1.163)
Least supportive of Putin polling station		-1.825***
		(0.333)
Sanctioned firm within 10km # Least supportive of Putin polling station		0.012
		(0.472)
Demographic controls	yes	yes
Economic controls	yes	yes
Subregion FE	yes	yes
R-squared	0.877	0.880
N	58763	58763

Notes: The unit of observation is polling station. The dependent variable is the percentage change in total votes cast between 2012 and 2018 presidential elections. The variable "Change in eligible voters (%), 2012-2018" indicates the percentage change in eligible voters registered at a given polling station between 2012 and 2018. The variable "Sanctioned firm within 10 km" is a binary variable that equals one if a sanctioned firm is within 10 km distance from the polling station. The dummy variables "Most supportive of Putin polling station" and "Least supportive of Putin polling station" are constructed by dividing the 58763 polling station observations into thirds based on Putin's vote share in the 2018 presidential elections. The variable "Most supportive of Putin polling station" indicates a polling station in the top third. The variable "Least supportive of Putin polling station" indicates a polling station in the bottom third. The omitted category indicates the middle third. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at polling station level in the 2012 presidential election.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The literature examining voters' support for Putin puts forward several explanations. One strand of research maintains the economic performance hypothesis, i.e., the Russian public assesses the performance of the political elite based on objective economic performance measures (Rose, Mishler and Munro, 2011; Treisman, 2011). Others underline the importance of control over media in Russia as well as the active hindrance of political challengers eager to join the political system (Enikolopov et. al., 2011, 2016; Robertson, 2017). In exploring the attitudinal changes of the electorate under extraordinary conditions one could distill the most likely foundations of a regime support. Sanctions imposition provide a useful opportunity to test whether in moments of international crisis Russians are prone to "rally-around-the-flag".

In order to discern between competing explanations for the increased support of Putin at polling stations featuring a sanctioned firm, I devise the following test. I collect employment data for the sanctioned firms, before and after the imposition of sanctions.[9] For every polling station, featuring more than one sanctioned firm within 10 km distance, I aggregate the number of employees of all the nearby sanctioned firms. I then split the treated polling stations in two groups – those where in sum, sanctioned firms within 10 km have gained additional employees between 2013 and 2017, and those where the sanctioned firms (in sum) have lost employees over the same period.

$$share_{ij,2018}^{Putin} - share_{ij,2012}^{Putin} = \alpha + \beta_1 T10_{ij,2018}^{empl.increase} + \beta_2 T10_{ij,2018}^{empl.loss} + \boldsymbol{B}_{2017}\boldsymbol{X}_{j,2017} + \boldsymbol{B}_{2017-2011}\boldsymbol{X}_{j,2017-2011} + \varepsilon_{ij} \quad (2.4)$$

Equation (2.4) is identical to the baseline specification with exception of the treatment variable, which is now split into treated polling stations where employment at sanctioned firms rose between 2013 and 2017, $T10_{ij,2018}^{empl.increase}$, and those polling stations where employment at sanctioned firms declined during

[9] Employment data for Russian firms is provided by Bureau van Dijk, AMADEUS database.

the same period, $T10_{ij,2018}^{empl.loss}$. By separately estimating the effects of winning and losing sanctioned firms, I can check whether the electorate blindly rallies around the flag or if it responds rationally to economic forces. Table 2.12 shows that only those sanctioned firms that continued to perform well after sanction imposition increase the vote share for Putin. The estimated coefficient for the sanctioned firms that lost employees between 2013 and 2017 is negative, albeit insignificant.

Table 2.12: ECONOMIC EXPLANATION

Dependent variable: Putin's vote share change between 2018 and 2012 presidential elections	
Employment gain at sanctioned firms, 2017-2013	0.868*
	(0.485)
Employment loss at sanctioned firms, 2017-2013	-0.203
	(0.548)
Demographic controls	yes
Economic controls	yes
Subregion FE	yes
R-squared	0.567
N	58763

Notes: The unit of observation is polling station. The dependent variable is (Putin's vote share for the 2018 presidential election) - (Putin's vote share for the 2012 presidential election). The variable "Employment gain at sanctioned firms, 2017-2013" measures the log gain in employment at sanctioned firms within 10 km distance from the polling station, between 2013 and 2017. The variable "Employment loss at sanctioned firms, 2017-2013" measures the log loss in employment at sanctioned firms within 10 km distance from the polling station, between 2013 and 2017. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by subregion. The observations are weighted by the number of eligible voters at the polling station level in the 2012 presidential election.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Putin's leadership in Russia. The results imply that support for the government is subject to positive economic performance. Russians do not blindly "rally-around-the-flag". Even in hybrid regimes politicians are held accountable for the economic performance of the country. Exposure to a sanctioned firm is

associated with higher levels of support for Putin, but only when those firms continue to perform well under the sanctions regime.

My empirical analysis provides several principal findings. First, I find evidence that exposure to a sanctioned firm is associated with increased levels of support for Putin. Moreover, I established that there has been a mobilization of nonvoters as well as switch of voters from the anti-Putin polling stations voting in favor of Putin in the 2018 elections, when there was a nearby sanctioned firm. This evidence is supportive of the "rally-around-the-flag" hypothesis, but as this section shows, it is an insufficient explanation. Specifically, sanctioned firms' performance measured by the percentage change in total employees between 2013 and 2017 is a strong and statistically significant predictor of higher support for Putin at the treated polling stations. The loyalty and increased support of the constituency is not irrational but involves sustained economic performance. I interpret these effects as supportive of the economic performance hypothesis (Rose, Mishler and Munro, 2011; Treisman, 2011).

6. Conclusion

This paper studies the political consequences of targeted sanction imposition on elections in Russia. In particular, I examine the impact of sanction imposition in 2014 on the change in Putin's vote share between 2012 and 2018. I find evidence that sanction imposition increased Putin's electoral approval by 1.54 percentage points. This shift is sizable compared to the 13.1 percentage point overall shift in support for Putin between 2012 and 2018. Heterogeneity results suggest that these electoral responses are stronger at those polling stations that show either relatively high but or particularly low support for Putin. Combined with the impact on turnout, I interpret the effect of sanctioned firms as mobilizing nonvoters in pro-Putin localities and persuading voters to switch in favor of Putin in anti-Putin areas.

Based on firm-level employment data on the sanctioned firms, I deepen the analysis by testing a possible explanation for the increased vote share in favor of Putin. I explore if voters react to the loss of local jobs induced by sanctions. I find that the upward shift in vote share for Putin is only statistically significant at those polling stations for which the nearby sanctioned firms experienced employment gains over the sanctions period. The lack of punitive reaction from voters in vicinity of sanctioned firms experiencing economic losses may indicate the acceptance of the narrative of Western responsibility and "rallying around the flag".

The findings have implications for understanding the impact of modern-day "smart" sanctions that were introduced precisely with the goal to affect only specific groups related to the ruling elite and prevent unintended consequences.

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APPENDIX A2

TABLE A2.1: PLACEBO REGRESSION AT THE SUBREGION LEVEL

Dependent variable: United Russia's vote share change between 2012 and 2008 presidential elections	
Sanctioned firm within subregion	0.688
	(0.470)
Demographic controls	yes
Economic controls	yes
Region FE	yes
R-squared	0.706
N	1816

Notes: The unit of observation is the subregion. The dependent variable is (United Russia's vote share for the 2012 presidential election) - (United Russia's vote share for the 2008 presidential election). The variable "Sanctioned firm within subregion" is a binary variable that equals one if a sanctioned firm is located within the boundaries of the subregion. The demographic controls are population, shares of males, elderly and social benefits recipients in the total population and rural population share, all measured at the subregion level and made available by Rosstat, present both in the 2017 values and in differences between 2017 and 2011. The economic controls are average wage, state investment in fixed assets per capita and goods and services produced, all measured at the subregion level and provided by Rosstat, used in the estimation both in 2017 values and in differences between 2017 and 2011. Robust standard errors are clustered by region. The observations are weighted by the number of eligible voters at the subregion level in the 2012 presidential election.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3: Strategic goods trade bias in human rights sanctions

I study whether there is a strategic goods trade bias in human rights sanctions. I exploit bilateral commodity-level data on trade between human rights-concerned countries and human rights violators between 1972 and 2005. I show that human rights-abusing countries exporting energy and nuclear products are sanctioned less often. Exports of nuclear goods and armaments by human rights-concerned countries to human rights abusers also mitigate the use of sanctions. Economic rationales and not geopolitical considerations explain the impact of strategic trade on sanction use.

JEL-Codes: F13, F51, N40

Keywords: human rights violations, sanctions, strategic goods, economic interdependence, double standards in international relations

1. Introduction

The United States Commission on International Religious Freedom has put both Saudi Arabia and Iran on its Tier 1 "Countries of Particular Concern" list.[10] Such classification calls for action by the US, the one most commonly applied being sanctions imposition. Of the two countries, only Iran is currently under a US sanction regime. Saudi Arabia, a long-standing, important US geopolitical ally and trade partner, has never been sanctioned by the US.

[10] http://www.uscirf.gov/all-countries/countries-of-particular-concern-tier-1

Sanctions are one of the strategies used in the international community for reducing human rights abuse (Hafner-Burton, 2014). Their ideal goal is to change a target country's behavior through the application of diplomatic and economic pressure. Human rights sanctions are imposed for a variety of reasons, ranging from political repression and religious persecution by a government, through state support for terrorists to instigation of politically and ethnically based murders and genocide. Beyond the officially stated or intended policy goals, human rights sanctions may have unwanted economic consequences, for both the sender and the target of sanctions. Sanctions inflict a "deadweight loss of utility" due to lost welfare benefits for all the parties involved (Pape, 1997; Eaton and Engers, 1999; Drezner, 2003). Specifically, sanctions may disrupt trade flows that are of importance to the countries involved (Hufbauer et. al., 1997).

Sanctions scholars have often simplified the impact of trade on sanctions by modelling it as a simple yes or no question: "Does the volume of bilateral trade reduce the probability of sanctions?". I claim that trade interdependence is more complex than the volume of bilateral trade employed in previous studies. In this paper, I examine whether sanctions are less likely to be initiated when – inter alia – strategic economic relations between the potential sender country and target country are stronger. Does the trade in strategic goods, such as natural resources, armaments or high tech goods decrease the likelihood of sanctions imposition?

I combine country-pair-level data on sanctions imposition with product-level bilateral trade flows, in a panel of 43 sanctioning countries and 91 human rights-violating countries between 1972 and 2005. I find that human rights-concerned countries turn more often a blind eye on human rights abuses in countries with which they engage in strategic goods trade. Human-rights-abusing countries are less likely to be sanctioned if they import nuclear materials and armaments or export energy and nuclear products from potential sanction senders. I then deepen the analysis by exploring alternative explanations for the mitigating impact of strategic goods trade on sanctions imposition. The differential treatment of human rights abusers may be due to rational economic calculations

or may be rooted in political concerns. My analysis shows that human rights-protecting countries sanction less often those states on which they *depend* more for trade with strategic goods. They also treat in a milder way strategic goods trade partners when the trade relationship is not easily *substitutable*. Finally, I find no evidence that strong *rivalry* in a strategic good market has an impact on sanctions initiation. When examining a possible political explanation, my findings reveal the a *geopolitical partnership*, measured by UN voting similarity between the potential sanction sender and potential sanction target, does not explain the impact of strategic trade on sanctions.

The literature relating trade to sanctions has not reached a consensus on the effect of trade on the decision to initiate a sanction. Several studies have found that trade reduces the probability of sanction initiation (Early, 2011; McLean and Whang, 2010). Others claim trade not to be relevant at all for the decision to initiate a sanction (Von Soest and Wahmann, 2015). I offer a fresh micro-data-driven answer to the question of how trade affects sanctions imposition by demonstrating that trade enters the sanction-making process in a more subtle way than the one researchers have previously employed. Whereas existing studies use total bilateral trade flows, I argue that it is not the volume of trade per se that governs the decision to sanction but the strategic nature of the trade pattern. The importance of a trade relationship depends on how valuable the goods being traded are to the trade partners. Losing a supplier of 1 billion dollars' worth of oil has different implications than losing a supplier of 1 billion dollars' worth of toys.

I also contribute to the literature linking economic interdependence – in terms of trade flows, capital flows, foreign direct investment and cross-border bank lending – to the use of sanctions in particular and motivation of sanctions imposition in general. To this end, scholars have identified the level of economic interdependence and the relative economic capabilities between a sender and a target as crucial for the decision to initiate a sanction (Cox and Drury, 2006; Hafner-Burton and Montgomery, 2008). There is also evidence that geopolitical

alignment and similarity of the political regimes between target and sender might affect the decision to initiate sanctions (Drezner, 1998; Drury, 2000; Lektzian and Souva, 2003; Cox and Drury, 2006; Goenner, 2007). My results support the hypothesis that economic interdependencies affect sanctions. While I do not find political proximity between countries to be relevant, I exploit the pattern of economic interdependence – dependence, substitutability and rivalry between the sender and the target – and show that those matter for sanctions imposition.

My results also speak to the literature exploring whether aid donors consider human rights abuses in the recipient states. There are some indications that human rights sanctions are rarely applied to countries that are geopolitically aligned with their donors (Tomasevski, 1997; Barratt, 2008). Yet others find that human rights protection is rewarded by aid donors (Cingraneli and Pasquarello, 1985; Poe, 1992; Berthelemy, 2006). Still, the overwhelming majority of contributions on human rights sanctions and aid find that strategic political considerations are at play (Neumayer, 2003; Rioux and Van Belle, 2005; Barratt, 2008). My paper shows that the relationship between human rights sanctions and trade is governed by economic concerns and not by political ones.

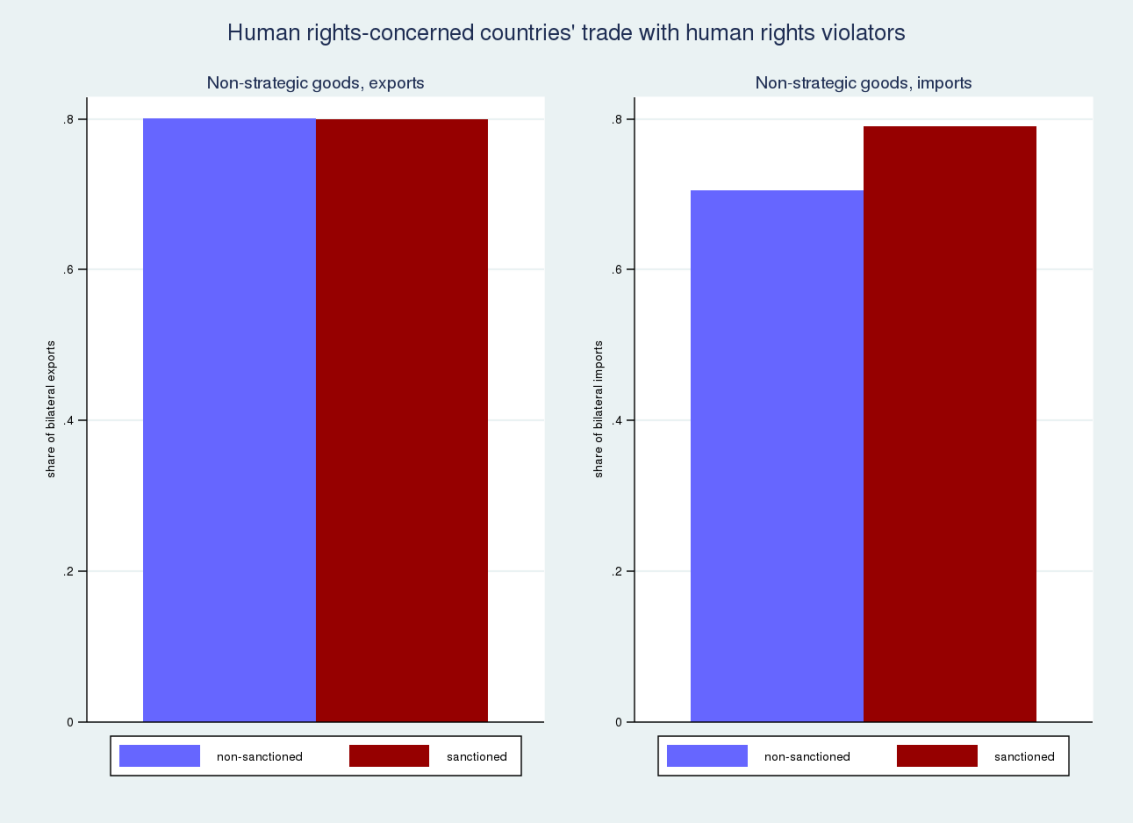
A simple comparison of strategic goods trade between human rights-promoting and human rights-abusing countries illustrates why one might perceive human rights sanctions as influenced by strategic goods trade. Human rights-promoting countries have exported relatively more non-ferrous metals, electronics, armaments and nuclear goods to human rights abusers that they have not sanctioned (Figure 3.1). On the other hand, exports of energy and chemicals have been relatively higher to those human rights-mistreating countries, which the human rights promoters have sanctioned. Imports of strategic goods are higher from non-sanctioned human rights violators for all strategic product groups but non-ferrous metals, i.e., energy, chemicals, electronics, nuclear goods and armaments (Figure 3.2).

FIGURE 3. 1: HUMAN RIGHTS-CONCERNED COUNTRIES' EXPORTS TO HUMAN RIGHTS VIOLATORS

FIGURE 3. 2: HUMAN RIGHTS-CONCERNED COUNTRIES' IMPORTS FROM HUMAN RIGHTS VIOLATORS

Whereas exports in non-strategic goods barely differ between the two groups, human rights-concerned countries import relatively less non-strategic goods from human rights-violating countries which they do not sanction (Figure 3.3).

FIGURE 3. 3: HUMAN RIGHTS CONCERNED COUNTRIES' TRADE WITH HUMAN RIGHTS VIOLATORS



This paper proceeds as follows. In section 2, I outline the nexus between human rights sanctions and strategic trade. Section 3 describes the data whereas section 4 and section 5 provide the econometric specification and the main result, respectively. Section 6 tests for possible explanatory mechanisms driving the main results whereas section 7 provides robustness checks. Section 8 concludes.

2. Background on human rights sanctions and trade in strategic products

Human rights sanctions take the form of either trade restrictions – import and export restrictions, blockades, quotas, licensing requirements – or other economic measures which may indirectly affect trade – termination of foreign aid, foreign assets freeze, conditions on government procurement, travel bans. Human rights sanctions have been employed for several purposes. They may be imposed to express the sender country's distaste for human rights abuses, to prevent a human rights-abusing state from acquiring needed goods, to punish a country for its human rights abuses or to generate pressure and alienate supporter countries of human rights-violating states.

However, human rights-protecting countries may apply sanctions selectively. Most obviously, countries may not engage in sanctioning of states with which they have strategic partnership, e.g., through a military alliance or through informal coalitions within international institutions such as the United Nations (Dreher et. al., 2008). Moreover, several studies link economic interdependence to the use of sanctions. While in some studies trade does have a mitigating role on the probability of sanction (Cox and Drury, 2006; Crescenzi, 2003; Drury et. al., 2014; Goenner, 2007; Hafner-Burton and Montgomery, 2008; Lektzian and Souva, 2003), it turns out to be not important in others (Von Soest and Wahman, 2015). These studies have typically used the total volume of bilateral trade or total bilateral imports or exports to proxy for economic interdependence between countries. This approach, however, does not account for the different inherent strategic value of some goods. A billion dollars' worth of toys imports from China are valued differently by the US than a billion dollars' worth of oil imports from Saudi Arabia. A disaggregation of the goods traded allows to uncover to which trade flow disruptions a country is indeed vulnerable (Gasiorowski, 1986). A country is more vulnerable to a trade breakdown if it has only a few trade partners or trades goods with a few substitutes. Hence, whether the interruption

of trade leaves a country vulnerable depends on the composition of trade and on the specific trade patterns.

The political science literature classifies strategic goods as those which are crucial for the economic and military strength of a country. There have been several contributions in the literature as regards to the definition and categorization of strategic goods. According to Sen (1984), strategic products ensure the self-sustained economic growth of a country. The author designates the following industries as strategic – iron and steel, chemicals, textiles, machinery, paper products and transport equipment – due to their backward and forward linkages to other industries, economies of scale and importance for the growth of the whole economy. Ripsman and Blanchard (1996) characterize products as strategic if they are crucial for survival, sourced from abroad, their share in the total trade volume of a country is high, and their substitutability is low. Goenner (2010) integrates these arguments to classify strategic goods as belonging to one of the following categories – energy, non-ferrous metals, chemicals, electronics, nuclear materials and armaments. These categories account for both scarcity of production or occurrence and are of importance to a nation's economic and military security. For example, non-ferrous metals and energy products are substantial production inputs for any economy and are at the same time concentrated among few suppliers. Other commodities, such as weapons and nuclear materials can provide military strength or threaten another country's existence and are likely to be traded only with geopolitically aligned countries. Non-ferrous metals are scarce in their occurrence and are indispensable for the production of steel, electronics, chemicals and durable goods. Energy products include oil, natural gas, coal and electricity. Chemicals include organic and inorganic chemicals. Electronic products include electrical machinery and apparatus, professional scientific and controlling instruments and telecommunications equipment. Nuclear goods contain radioactive materials as well as nuclear reactors and parts. Armaments consist of weapons, armored vehicles, warships, aircraft and helicopters. I follow Goenner's (2010) strategic goods classification in my analysis.

Decomposing trade in strategic and non-strategic accounts for the content of trade and its impact on sanctions. But it does not answer the question as to why exactly particular products may or may not influence the decision to initiate a sanction. Countries trade in strategic goods also partly in order to influence the behavior of other countries and to create dependencies. Thus, geopolitical allies may trade more intensively in strategic goods and also sanction each other less often. On the other hand, (not) sanctioning because of trade may be driven by purely economic considerations. Countries may differ in the degree of dependence they have on a given strategic good supplier. Strategic goods also vary in the degree of their production and occurrence concentration, and hence their trade partner substitutability. Moreover, states exposed to fierce competition in a strategic good market may be less eager to cut off existing trade ties. Hence, in section 6 I check whether the political interaction within a country pair does a better job at explaining the impact of trade on sanctions than economic interdependencies.

3. Data

To test whether there is a strategic trade bias in the decision to initiate a sanction case against a human rights-abusing country, I combine country-pair-level data on sanctions imposition with product-level trade flows between 43 sanction-initiating countries and 91 human rights violators between 1972 and 2005.

3.1 Dependent variable: human rights sanction case

Data on human rights sanctions comes from the Threat and Imposition of Economic Sanctions dataset, by Morgan, Bapat and Kobayashi (2014). The data has been collected via human search in Lexis-Nexis, Facts on File, Keesing's Record of World Events, the New York Times index, and the London Times index. Sanctions are defined as actions a country undertakes to reduce or cut its economic ties with a target country, aiming thereby to change the target country's

behavior or policies. I use sanctions case data from 1972 until 2005, which renders a total of 167 sanction cases. My dependent variable is human rights sanction case start, which implies either the imposition of a sanction or the official declaration of a sanction threat.

A sanction-initiating country is a country that has initiated at least one human rights sanction case throughout the sample period. Table 3.1 reveals that the US has a remarkable dominance as a human rights sanction sender in the international community.

TABLE 3. 1: HUMAN RIGHTS SANCTIONS SENDERS

Sanction sender	**Frequency**	**%**
United States of America	54	32.34
United Kingdom	14	8.38
France	13	7.78
Germany	12	7.19
Canada	11	6.59
Japan	6	3.59
Australia	5	2.99
Libya	3	1.80
Iraq	3	1.80
Saudi Arabia	3	1.80
Kuwait	3	1.80
Belgium	2	1.20
Spain	2	1.20
Italy	2	1.20
Russia	2	1.20
Sweden	2	1.20
Algeria	2	1.20
Syria	2	1.20
New Zealand	2	1.20
Venezuela	1	0.60
Argentina	1	0.60
Ireland	1	0.60
Netherlands	1	0.60
Switzerland	1	0.60
Czechoslovakia	1	0.60
Albania	1	0.60
Bosnia and Herzegovina	1	0.60
Greece	1	0.60
Finland	1	0.60
Norway	1	0.60
Denmark	1	0.60
Nigeria	1	0.60
Central African Republic	1	0.60

Chad	1	0.60
Congo	1	0.60
Democratic Republic of the Congo	1	0.60
Kenya	1	0.60
Tanzania	1	0.60
Burundi	1	0.60
Rwanda	1	0.60
Ethiopia	1	0.60
China	1	0.60
India	1	0.60
Total	167	100.00

For each year, I define a country to be a human rights violator if the country has received the rating "partly free" or "not free" by the human rights-specializing non-governmental organization Freedom House. This indicates that a country has engaged in considerable violations of either political rights – electoral process, political pluralism and participation and functioning of the government – or of civil liberties – freedom of expression and belief, associational and organizational rights, rule of law and personal autonomy and individual rights (Freedom House, 2018). The sample of human rights-violating countries, ever sanctioned or not, appears in Table 3.2.

As data on human rights violations is available only after 1972, I restrict my analysis to the post-1972 period. Sanctions data is available only up until 2005 and thus provides the upper time limit of my study.

TABLE 3. 2: HUMAN RIGHTS-VIOLATING COUNTRIES

Sanctioned countries	**Sanction frequency**	**Non-sanctioned countries**
Zimbabwe	16	United States
Israel	12	Cuba
Haiti	10	Dominican Republic
Uganda	10	Mexico
Burundi	8	Honduras
China, P.R.: Mainland	7	Panama
Portugal	6	Colombia
Turkey	6	Venezuela, Republica Bolivariana de
Vietnam	6	Suriname
Peru	5	Ecuador
Chile	5	Hungary
Yugoslavia	5	Czechoslovakia

South Africa	5	Albania
Cambodia	5	Croatia
Congo, Democratic Republic of	4	Bosnia and Herzegovina
Indonesia	4	Georgia
Guatemala	3	Azerbaijan, Republic of
Togo	3	Equatorial Guinea
Nigeria	3	Senegal
Bangladesh	3	Mauritania
Russian Federation	2	Guinea
Cote d'Ivoire	2	Liberia
Ethiopia	2	Sierra Leone
Iraq	2	Cameroon
India	2	Central African Republic
Myanmar	2	Chad
El Salvador	1	Congo, Republic of
Nicaragua	1	Kenya
Brazil	1	Rwanda
Bolivia	1	Somalia
Paraguay	1	Eritrea
Argentina	1	Angola
Uruguay	1	Mozambique
Sudan	1	Namibia
Afghanistan, Islamic Republic of	1	Morocco
Uzbekistan	1	Algeria
Pakistan	1	Libya
Sri Lanka	1	Iran, Islamic Republic of
Thailand	1	Egypt
		Syrian Arab Republic
		Lebanon
		Yemen
		Yemen, P.D. Rep.
		Kuwait
		Tajikistan
		Korea, Democratic People's Rep. of
		Bhutan
		Nepal
		Lao People's Democratic Republic
		Malaysia
		Philippines
		Papua New Guinea

3.2 Bilateral trade flows

Data on bilateral trade flows on a product level has been originally collected by the United Nations and is made available through the UN COMTRADE database. I use a modified version of this original database with corrections made by Hausmann et. al. (2014). It includes exports and imports on a bilateral basis between all countries in the world, classified using the Standard Industry Trade

Classification (SITC), revision 2, at the four-digit level, between 1962 and 2017. By using product-level trade data, I can attend to the composition of trade and its strategic value to the trading countries. In order to classify products as strategic or non-strategic I rely on categorization provided by Goenner (2010), shown in Table 3.3.

TABLE 3. 3: STRATEGIC PRODUCTS CLASSIFICATION

Strategic category	**Commodity SITC Code**	**Description**
Energy	32	Coal, coke and briquettes
	33	Petroleum, petroleum products and related material
	34	Gas: natural and manufactured
	35	Electric current
Non-ferrous metals	287	Ores & concentrates of base metals n.e.s.
	681	Silver, platinum & other metals of the platinum group
	682	Copper
	683	Nickel
	684	Aluminum
	685	Lead
	686	Zinc
	687	Tin
	689	Misc. non-ferrous base metals employed in metallurgy
Chemicals	51	Organic chemicals
	522	Inorganic chemical elements, oxides and halogen salts
	523	Other inorganic chemicals
Electronics	87	Professional, scientific and controlling instruments
	764	Telecommunications equipment and parts
	77	Electrical machinery, apparatus and appliances n.e.s.
Nuclear	286	Ores and concentrates of uranium and thorium
	524	Radio-active and associated materials
	7187	Nuclear reactors and parts
Armaments	792	Aircraft and associated equipment and parts
	7931	Warships of all kinds
	9510	Armored fighting vehicles, arms of war and ammunition

3.3 Control variables

Since human rights sanctions are driven by both economic and political considerations, I make extensive use of trade and politics-related variables. I include the number of GATT/WTO members and the number of trade disputes in a country pair. International trade organizations give a platform for members to sort out any trade policy-related frictions, hence I expect that membership of both countries within the GATT/WTO would decrease the probability of sanction. Being engaged in a trade dispute signals a deterioration of trade relations which may facilitate trade disruption through sanction initiation. Data for these two variables came from the WTO. Then, to account for the influence of preferential trade agreements on both bilateral trade flows and the probability to sanction, I use the Economic Integration Agreements data by Baier and Bergstrand (2016) which runs from 1949 till 2016. Other variables accounting for bilateral trade facilitation and bilateral political relations such as contiguity and colonial links are taken from the CEPII bilateral distance database.

The level of democracy in each country is measured by a democracy index provided by the Polity IV Project. Additionally, I control for the level of human rights abuses in the following two ways. First, I include an index measuring the level of human rights abuses in a country for each year. The index comes from the Freedom House database. Second, I include an event-based variable – political or ethnic-based murders in a country for a given year – to account for spikes in human rights abuses which may be more recognizable in the international community. The data for this variable comes from the Political Instability Task Force.

Finally, I control for upcoming elections as foreign policy topics such as sanctions tend to gain particular attention in electoral years. Data on those comes from the National Elections Across Democracy and Autocracy dataset by Hyde and Marinov (2017).

4. Modeling the determinants of human rights sanctions

I examine whether there is a strategic product bias in the decision to initiate a human rights sanction case. For this purpose, I estimate the determinants of human rights sanction $HR\_sanction_{ijt}$ using OLS:

$$\begin{aligned} Pr(HR\_sanction_{ijt}) &= \beta_0 + \beta_1 strategic_{ijk} + \beta_2 trade_{ijk,t-1} \\ &+ \beta_3\, strategic_{ijk} * trade_{ijk,t-1} + X_{ij(t-1)} + Q_{it-1} \\ &+ Z_{jt-1} + \alpha_i + \tau_t + \varepsilon_{ijt} \end{aligned} \tag{3.1}$$

where $strategic_{ijk}$ is a categorical variable classifying products according to the seven strategic goods categories following Goenner (2010): energy, non-ferrous metals, chemicals, electronics, nuclear, armaments and non-strategic products. The variable $trade_{ijk,t-1}$ stands in for either $exports_{ijk,t-1}$ or $imports_{ijk,t-1}$, i.e., for bilateral export and import flows at the product level; $X_{ij(t-1)}$ is a vector that contains time-varying and time-invariant bilateral economic and political control variables; Q_{it-1} and Z_{jt-1} are vectors representing time-varying control variables for the human rights-concerned and human rights-violating countries respectively, α_i are human rights-concerned country-fixed effects, τ_t are year-fixed effects and ε_{ijt} is the error term. The errors may be correlated at the sanction-sending country-year level, which is why I allow for clustering at that level. The sample runs from 1972 to 2005.

My identification strategy exploits the following three sources of variation in the dependent variable: (1) difference in the sanction probability between human rights-abusing countries, (2) changes in sanction probability within human rights-abusing countries, and (3) differences in sanction probability between human rights-concerned countries for the same human rights-violating country at a given point in time. Since sanction occurrence in the data set is quite sparse (1.8%), I choose specification (3.1) as my baseline in order to identify the coefficients based on all three sources of variation.

5. Results

5.1 Benchmark

To test whether human rights sanctions show evidence for strategic product bias, I interact bilateral exports and imports with a strategic product indicator. Table 3.4 provides the main OLS results.

Exports of non-strategic goods from human rights-concerned countries to human rights-abusing countries increase the probability of human rights sanction. This can be seen from the positive coefficient on *log trade, sender to target, t-1* in column (1), which is statistically significant at the 5% level. A 100% increase in non-strategic exports increases the probability of human rights sanction by 0.09 percentage points. The increase in sanction probability becomes more pronounced for chemicals and electronics. For chemicals, a 100% increase in exports will increase the probability of human rights sanction by 0.17 percentage points relative to exports in non-strategic goods. The total effect for chemicals is equal to 0.26 percentage points increase in sanction probability (0.09+0.17). A 100% increase in exports in electronics will increase the probability of human rights sanction by 0.06 percentage points relative to exports of non-strategic goods. The total effect for electronic goods is equal to a 0.15 percentage points increase in sanction probability (0.09+0.06).

TABLE 3. 4: STRATEGIC PRODUCTS BIAS IN HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
log trade, sender to target, t-1	0.0009**	0.0016**
	(0.0004)	(0.0006)
Energy # log trade, sender to target, t-1	0.0001	-0.0026**
	(0.0004)	(0.0011)
Non-ferrous metal # log trade, sender to target, t-1	0.0004	-0.0014
	(0.0005)	(0.0009)
Chemicals # log trade, sender to target, t-1	0.0017***	0.0014*
	(0.0006)	(0.0008)
Electronics # log trade, sender to target, t-1	0.0006**	-0.0005
	(0.0003)	(0.0004)
Nuclear # log trade, sender to target, t-1	-0.0022**	-0.0026*
	(0.0011)	(0.0015)
Armaments # log trade, sender to target, t-1	-0.0013**	-0.0008
	(0.0006)	(0.0011)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.32	0.41
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. * $p < 0.10$, ** $p < 0.05$,

Whereas higher exports of the human-rights concerned countries to the human-rights violating countries and the probability of sanction are positively associated for non-strategic goods, chemicals and electronics, this relationship is less strongly pronounced for exports of nuclear goods and armaments. Exports of these two type of goods actually lower the probability of sanction. The total effect for nuclear goods is equal to -0.13 percentage points decrease in probability to sanction (0.09-0.22), whereas the total effect for armaments is equal to -0.04 percentage points decrease in sanction probability (0.09-0.13).

What is the impact of imports of human-rights concerned countries from human-rights abusing countries on the likelihood of the former to initiate a sanction case? These results are presented in the second column of Table 3.4. The higher the imports of non-strategic goods, the higher the sanction likelihood. A 100% increase in non-strategic imports will increase the probability of human rights sanction by 0.16 percentage points. This positive association is even more pronounced for imports of chemicals. Chemicals increase the probability for a sanction by a total of 0.30 percentage points (0.16+0.14).

Energy and nuclear product imports have an even stronger pronounced, negative effect on the probability of sanction. As compared to imports of non-strategic goods, sanctioning countries are on average 0.26 percentage points less likely to initiate a sanction if imports of energy products increase by 100%. This implies that the total effect of energy imports on sanction probability is -0.16 percentage points (0.10-0.26), thus reversing the positive relationship between imports and sanctions. The same is true for nuclear goods. Their imports lower the probability for a sanction initiation by 0.26 percentage points (for an increase by 100% in nuclear imports). The total effect of nuclear imports on sanction likelihood is thus equal to -0.16 percentage points (0.10-0.26).

Taken together, this is strong evidence in favor of strategic product trade partners receiving a milder treatment for human rights abuses. This is true for importers of nuclear and military products and exporters of energy and nuclear products. Of note is also the fiercer attitude towards importers of chemicals and electronics and exporters of chemicals. This may imply that these products are not considered strategic by the sanctioning country, either because it is capable of producing them by itself or due to the existence of adequate further suppliers of these goods. I provide some insights on these hypotheses in section 6.

5.2 Non-linear model

My regressions so far estimate equation (3.1) as a linear probability model. To test whether my results are robust to a non-linear specification, Table (3.5) presents results from a pooled probit estimation. They are qualitatively similar to the OLS results. Whereas exports of chemicals and electronics increase the probability of a sanction initiation, nuclear and military goods exports decrease the likelihood of a sanction. Imports of energy and nuclear products are also negatively associated with the probability of a sanction.

TABLE 3. 5: STRATEGIC PRODUCTS BIAS IN HUMAN RIGHTS SANCTIONS, POOLED PROBIT ESTIMATION

	Exports	Imports
	(1)	(2)
Log trade, sender to target, t-1	0.0209***	0.0243***
	(0.0077)	(0.0080)
Energy # log trade, sender to target, t-1	0.0008	-0.0384**
	(0.0088)	(0.0178)
Non-ferrous metal # log trade, sender to target, t-1	0.0063	-0.0233
	(0.0092)	(0.0146)
Chemicals # log trade, sender to target, t-1	0.0359***	0.0137
	(0.0104)	(0.0094)
Electronics # log trade, sender to target, t-1	0.0134**	-0.0077
	(0.0062)	(0.0079)
Nuclear # log trade, sender to target, t-1	-0.0462**	-0.0514**
	(0.0226)	(0.0227)
Armaments # log trade, sender to target, t-1	-0.0283**	-0.0144
	(0.0126)	(0.0228)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. * $p < 0.10$, ** $p < 0.05$,

5.3 US specification

By pooling all human rights-concerned countries in one regression I have so far implicitly assumed that each country weighs all human rights-abusing countries-specific factors in the same way, as I was estimating one single coefficient per explanatory variable. However, one single country – USA – makes for around 30% of all human rights sanction case initiations and hence may differ in its attitude towards human rights abuses from other, more sporadically sanctioning

countries. Therefore, I run an individual regression for the US and another one for the other sanction-initiating countries. Table 3.6 provides insights in the sanctions motivation for the US. The table highlights that only two types of products are relevant for the sanction decision of the US – armaments and energy products. Whereas a 100% increase in arms exports decreases the probability of sanction against a human rights-violating country by 0.20 percentage points, US energy imports from such countries decrease the sanction likelihood by 0.54 percentage points, relative to nonstrategic exports and imports. The total effects are then equal to -0.08 percentage points for arms exports and -0.22 percentage points for energy imports.

TABLE 3. 6: STRATEGIC PRODUCTS BIAS IN HUMAN RIGHTS SANCTIONS: US VS. NON-US SENDER

	Sender: US		Sender: Non-US	
	Exports	Imports	Exports	Imports
	(1)	(2)	(3)	(4)
Log trade, sender to target, t-1	0.0012	0.0032*	0.0008*	0.0010*
	(0.0007)	(0.0016)	(0.0004)	(0.0005)
Energy # log trade, sender to target, t-1	0.0002	-0.0054*	0.0003	-0.0016
	(0.0008)	(0.0030)	(0.0005)	(0.0010)
Non-ferrous metal # log trade, sender to target, t-1	-0.0006	-0.0035	0.0010*	-0.0002
	(0.0009)	(0.0023)	(0.0006)	(0.0007)
Chemicals # log trade, sender to target, t-1	0.0022	0.0001	0.0014**	0.0016*
	(0.0013)	(0.0011)	(0.0006)	(0.0009)
Electronics # log trade, sender to target, t-1	0.0014	0.0003	0.0003	-0.0009***
	(0.0008)	(0.0010)	(0.0003)	(0.0003)
Nuclear # log trade, sender to target, t-1	-0.0024	-0.0023	-0.0017	-0.0031**
	(0.0017)	(0.0035)	(0.0015)	(0.0015)
Armaments # log trade, sender to target, t-1	-0.0020*	-0.0006	-0.0008	-0.0007
	(0.0012)	(0.0018)	(0.0006)	(0.0010)
Non-interacted main explanatory variables	yes	yes	yes	yes
Control variables	yes	yes	yes	yes
Sender FE	yes	yes	yes	yes
Year dummies	yes	yes	yes	yes
R-squared	0.39	0.34	0.23	0.32
N	487,788	183,025	1,171,370	462,242

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (columns (1)) and (3) and log imports of sender from target (columns (2) and (4)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results for the other countries (all but the US) show preference for different strategic goods. Column (3) and (4) of Table 3.6 reveal that both non-ferrous metals and chemicals exports increase the probability for a human rights sanction, beyond the positive effect of non-strategic exports on sanction probability. On average, a 100% increase in the exports of non-ferrous metals

leads to a total increase in sanction probability of 0.18 percentage points (0.08+0.10). For chemicals this value is 0.22 percentage points (0.08+0.10). Imports of chemicals from human rights-abusing countries also increase sanction probability, whereas imports of electronics and nuclear products have a negative effect, as compared to non-strategic imports. Of these last two, only imports of nuclear products are so strongly negatively associated with sanction likelihood as to reverse the positive association of imports and sanction probability into a negative one. The total effect for nuclear products is equal to -0.21 percentage points (0.10-0.31). The total effect for electronics is positive and equal to 0.01 percentage points (0.10-0.09).

Taken together, the results from Table 3.6 indicate that both the US and the group of the occasionally sanctioning countries are influenced by strategic products trade in their decision to initiate a sanction case. Given that for different countries different products are of strategic importance, it is only natural that I find differences in the strategic products being statistically significant among the results for the US and the other countries.

Overall, I reject the null hypothesis of unbiased human rights sanctions. Countries sanction less often human rights violators that deliver certain strategic goods – specifically nuclear and energy products. Human rights-concerned countries also turn more often a blind eye on human rights abuses in countries to which they export armaments and nuclear goods. Exports and imports of chemicals and electronics actually increase the likelihood of a sanction.

6. Exploration of transmission channels

This section explores the channels of transmission of the strategic trade bias in a more detailed way. Both economic and geopolitical reasons may be behind the observed effect of strategic trade on the decision to sanction, hence these are going to be examined in the following subsections.

Human rights sanctions may harm a sanction sender's export and import interests. In particular, I discriminate between trade dependence, market rivalry and substitutability of the trade relationship as well as non-economic factors as moderating the effect of strategic trade on human rights sanctions.

6.1 Dependence

If production in a sanctioning country strongly depends on inputs from another country or a large share of its exports in a strategic product are absorbed by a single market, then the potential sanction sender may be less eager to initiate a sanction case against that particular country. These import and export *dependencies* are the primary potential risks that stem from a disruption of trade flows with a strategic trade partner.

In order to measure the dependence of a human rights concerned country from human rights-violating country in terms of exports and imports of strategic goods I use the following two measures. First, I measure to what degree the human rights-violating country is an important import or export partner of the sanctioning country for a particular strategic good. Specifically, I use the following import and export ratios:

$$\frac{exports_{ijkt-1}}{exports_{ikt-1}}$$

$$\frac{imports_{ijkt-1}}{imports_{ikt-1}}$$

where $exports_{ijkt-1}$ measures the exports of a sender country *i* of product *k* to target country *j* in *t-1* and $imports_{ijkt-1}$ accounts for the imports of a sender country *i* of a product *k* from target country *j* in *t-1*.

These two trade ratios allow to account for the relative importance of a trade partner for each strategic product. They measure the previous period dependence of a sanctioning country from its potential target. On the basis of these indicators, I can account for the vulnerability of a potential sanction sender to a disruption in imports or exports of strategic goods.

Rerunning specification (3.1) with these export and import ratios instead of logged bilateral import and export flows reveals suggestive evidence on the role of trade interdependencies for the sanction decision (Table 3.7). The higher the dependence of the potential sanction sender from the potential sanction target as a destination for its armaments and nuclear products, the lower the probability of sanction. A change in the nuclear products exports ratio by 0.10 is estimated to decrease the probability for a human rights sanction by 1.3 percentage points ((0.40-0.53)*0.10*100). An increase in the armaments exports ratio by 0.10 will decrease the probability of sanction by 0.2 percentage points ((0.40-0.42)*0.10*100).

TABLE 3. 7: TRADE PARTNER IMPORTANCE AND HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
Target importance as export market/import supplier, t-1	0.4096	0.7066*
	(0.2819)	(0.4246)
Energy # Target importance as export market/import supplier, t-1	-0.1304	-0.7872*
	(0.2133)	(0.4191)
Non-ferrous metal # Target importance as export market/import supplier, t-1	-0.1616	-0.7073*
	(0.1427)	(0.4013)
Chemicals # Target importance as export market/import supplier, sender to target, t-1	0.0336	-0.4391*
	(0.1468)	(0.2439)
Electronics # Target importance as export market/import supplier, sender to target, t-1	-0.2180	-0.7337**
	(0.1685)	(0.2959)
Nuclear # Target importance as export market/import supplier, sender to target, t-1	-0.5306**	-0.7853*
	(0.2654)	(0.4167)
Armaments # Target importance as export market/import supplier, sender to target, t-1	-0.4246*	-0.8513**
	(0.2479)	(0.3269)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.28	0.29
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. In column (1) the regression includes the variable "Target importance as export market " and its interactions, in column (2) the regression includes the variable "Target importance as import supplier" and its interactions. Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target

Turning to import dependence, armaments, electronics, energy and nuclear products have a depressing effect on the likelihood for a sanction initiation. An increase by 0.10 in the import dependence for armaments of the human rights-concerned country from the human rights-abusing country decreases the probability for sanctioning by 1.5 percentage points ((0.70-0.85)*0.10*100).

Analogous calculations of the total effects yield a 0.8 percentage points decrease for nuclear products, 0.8 percentage points decrease for energy products and 0.3 percentage points decrease for electronics. While chemicals and non-ferrous metals have a mitigating effect of the overall positive effect of imports dependence in non-strategic products on the probability for a sanction, it is not as strong as to reverse the relationship into a negative effect on sanction probability.

A further way to measure export and import dependence comes with the Balassa index. According to the comparative advantage theory which is central to the standard Ricardian or Heckscher-Ohlin models of trade, countries increasingly export the goods they have a relative advantage in producing and import those goods they have a relative disadvantage in producing. To test to what extent comparative advantage and disadvantage in strategic products trade are determining the decision to initiate a human rights sanction, I need a measure of the sender country's competitiveness in exporting or disadvantage in importing a strategic product. To this end, I use Balassa's (1965) measure of revealed comparative advantage:

$$RCA_{ikt-1} = \frac{\frac{exports_{ikt-1}}{exports_{it-1}}}{\frac{exports_{kt-1}}{exports_{t-1}}}$$

where $exports_{ikt-1}$ defines the total exports of country *i* in a strategic product *k* in year *t-1*. The revealed comparative advantage index measures the degree of specialization of a country in a given product and is a ratio of two ratios. The numerator represents country *i*'s share of world exports in product *k*. The denominator stands for the share of product *k* in total world exports. Hence, Balassa index compares a country's share of total world exports in product *k* to the aggregate world export share of product *k*. If the ratio is bigger than one,

then the country captures a greater share of world exports in product *k* than the global average, which implies that the country has a comparative advantage in producing product *k*.

A modified version of Balassa's revealed comparative advantage index can be used to account for the world import share of a country, i.e., the revealed comparative dependence of a country for imports of a given product:

$$RCD_{ikt-1} = \frac{\frac{imports_{ikt-1}}{imports_{it-1}}}{\frac{imports_{kt-1}}{imports_{t-1}}}$$

where, analogously to above, $imports_{ikt-1}$ stand for the total imports of country *i* in a strategic product *k* in year *t-1*, $imports_{it-1}$ are country *i*'s total imports, $imports_{kt-1}$ are total world imports of product *k* and $imports_{t-1}$ are total world imports. A value of the index greater than one means that in that product country *i* imports relatively more than the aggregate global average import share. This implies that for the supply of that particular product country *i* is more dependent on imports than the world on average.

Using the constructed measures from above, I test whether an exporter's comparative advantage in exporting a strategic product or an importer's comparative dependence in importing a strategic product influence the human rights sanction probability. The specification is as follows:

$$
\begin{aligned}
&Pr(HR\_sanction_{ijt}) \\
&\quad = \beta_0 + \beta_1 strategic_{ijk} + \beta_2 trade_{ijk,t-1} \\
&\quad + \beta_3 RC\#_{ijk,t-1} + \beta_4\, strategic_{ijk} * trade_{ijk,t-1} \\
&\quad + \beta_5\, strategic_{ijk} * RC\#_{ijk,t-1} + \beta_6 trade_{ijk,t-1} \\
&\quad * RC\#_{ijk,t-1} \\
&\quad + \beta_7 strategic_{ijk} * trade_{ijk,t-1} * RC\#_{ijk,t-1} \\
&\quad + X_{ij(t-1)} + Q_{it-1} + Z_{jt-1} + \alpha_i + \tau_t + \varepsilon_{ijt}
\end{aligned}
\tag{3.2}
$$

The variable $trade_{ijk,t-1}$ stands in for either $exports_{ijk,t-1}$ or $imports_{ijk,t-1}$, i.e., for bilateral export and import flows at the product level and the variable $RC\#_{ikt-1}$ stands in for either RCA_{ikt-1} or RCD_{ikt-1}. For ease of interpretation, the Balassa indices RCA_{ikt-1} and RCD_{ikt-1} have been dichotomized, i.e., for values equal to or above one the indices have been assigned the value 1 and for values smaller than one, the indices have been assigned a value of 0.

In specification (3.2) I allow the effect of strategic goods imports and exports to differ depending on the extent to which the exporting country has a comparative advantage in product *k* or the importing country has a comparative dependence on product *k*. If the impact of strategic trade on sanctions is due to comparative advantage or comparative dependence in a given product, I expect the three-way interaction terms to be statistically significant.

Estimates of specification (3.2) are reported in Table 3.8. A country's competitiveness is an important factor for the decision to sanction when the sanctioning country is exporting non-ferrous metals and armaments. Having a comparative advantage in nonferrous metals or armaments significantly reduces the probability to initiate a human rights sanction case. The three-way interactions with relative important dependence are not statistically significant. Taken together, these results suggest that export dependence is particularly important for non-ferrous metals and armaments whereas import dependence is not affecting the probability to initiate a human rights sanction.

TABLE 3. 8: SENDER'S COMPARATIVE ADVANTAGE/DEPENDENCE AND HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	0.0007***	-0.0002
	(0.0003)	(0.0003)
Energy # Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	-0.0003	0.0009
	(0.0008)	(0.0010)
Non-ferrous metal # Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	-0.0017*	0.0003
	(0.0009)	(0.0012)
Chemicals # Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	-0.0003	-0.0012
	(0.0007)	(0.0009)
Electronics # Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	0.0009	-0.0001
	(0.0006)	(0.0005)
Nuclear # Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	-0.0020	0.0057
	(0.0021)	(0.0035)
Armaments # Comparative advantage/dependence of sender, t-1 # Log trade, sender to target, t-1	-0.0025**	0.0020
	(0.0012)	(0.0018)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.32	0.41
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). In column (1) the regression includes the variable "Comparative advantage of sender" and its interactions, in column (2) the regression includes the variable "Comparative dependence of sender" and its interactions. Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. *

In sum, a bilateral dependence of the potential sender from the potential target has been shown to be important sanction factor for several strategic products and for both imports and exports. In contrast, the dependence of the potential sender on the imports and exports of a particular strategic product as measured by the Balassa indices has delivered only sparse evidence for impact, specifically in the product groups non-ferrous metals and armaments and only for exports.

6.2 Substitutability

Not every sanction comes along with the same price tag. The potential effect from loss of trade is larger when the trade partner is not easily *substitutable*. The opportunity costs of sanctioning would be higher and the human rights-propagating country reluctant to initiate a sanction in such a case. Especially for strategic goods, some countries are hardly substitutable as suppliers of imports or destinations for exports, while other trade partners of non-strategic goods can easily be replaced. Hence, I need a measure of the substitutability of a trade relationship.

To assess the degree of substitutability, the following two measures are computed: the world market share of each trade partner in each product and the number of countries that export or import each product as a share of the total number of countries in the world. The second measure is referred to as ubiquity, borrowing from the work on product ubiquity by Hidalgo and Hausmann (2010). The world export and import shares can be expressed in the following simple way:

$$export\ share_{jk,t-1}^{world} = \frac{exports_{jkt-1}}{exports_{kt-1}}$$

$$import\ share_{jk,t-1}^{world} = \frac{imports_{jkt-1}}{imports_{kt-1}}$$

where $exports_{jkt-1}$ measures the total exports of a potential target in a product *k* in *t-1* and $exports_{kt-1}$ measures the total world exports in product *k* in year *t-1*. Further, import and export ubiquity is defined as:

$$ubiquity_{k,t-1}^{ex} = \frac{number\ of\ exporters_{t-1}}{number\ of\ countries_{t-1}}$$

$$ubiquity_{k,t-1}^{im} = \frac{number\ of\ importers_{t-1}}{number\ of\ countries_{t-1}}$$

To test whether the degree of substitutability of strategic products trade partners has an effect on the probability of human rights sanction, the following specifications are employed:

$$\begin{aligned} Pr(HR\_sanction_{ijt}) &= \beta_0 + \beta_1 strategic_{ijk} + \beta_2 exports_{ijk,t-1} \\ &+ \beta_3 export\ share_{jk,t-1}^{world} + \beta_4\ strategic_{ijk} \\ &* exports_{ijk,t-1} + \beta_5\ strategic_{ijk} \\ &* export\ share_{jk,t-1}^{world} + \beta_6 exports_{ijk,t-1} \\ &* export\ share_{jk,t-1}^{world} \\ &+ \beta_7 strategic_{ijk} * exports_{ijk,t-1} \\ &* export\ share_{jk,t-1}^{world} + X_{ij(t-1)} + Q_{it-1} + Z_{jt-1} + \alpha_i \\ &+ \tau_t + \varepsilon_{ijt} \end{aligned} \tag{3.3}$$

$$
\begin{aligned}
&Pr(HR\_sanction_{ijt}) \\
&\quad = \beta_0 + \beta_1 strategic_{ijk} + \beta_2 exports_{ijk,t-1} \\
&\quad + \beta_3 ubiquity^{ex}_{k,t-1} + \beta_4\, strategic_{ijk} * exports_{ijk,t-1} \\
&\quad + \beta_5\, strategic_{ijk} * ubiquity^{ex}_{k,t-1} + \beta_6 exports_{ijk,t-1} \\
&\quad * ubiquity^{ex}_{k,t-1} \\
&\quad + \beta_7 strategic_{ijk} * exports_{ijk,t-1} * ubiquity^{ex}_{k,t-1} \\
&\quad + X_{ij(t-1)} + Q_{it-1} + Z_{jt-1} + \alpha_i + \tau_t + \varepsilon_{ijt}
\end{aligned}
\tag{3.4}
$$

The specifications dealing with world import share and ubiquity of importers are constructed in analogous way. The results from these estimations are provided in Tables 3.9 and 3.10. The degree of substitutability of a trade partner as an export market is statistically significant for non-ferrous metals and electronics when substitutability is measured by world import and exports shares of the target. In these product groups, a higher world import share of a potential target country will lower the sanction probability by a potential sanction-initiating country which exports these product groups. On the import side, a human rights-promoting country which imports non-ferrous metals from a human rights-violating country would be less eager to initiate a sanction if the potential target holds a large share of the world non-ferrous metal exports. When accounting for ubiquity of exporters and importers, a statistically significant relationship exists for electronics exports. If there are numerous other importers of electronics, then a human rights-concerned country will be more likely to initiate a sanction against a human rights violating country to which it exports electronics. When it comes to imports, results are less intuitive. A higher number of exporters decreases the probability of sanction for a potential sanction sender importing chemicals from a potential sanction target.

TABLE 3. 9: TARGET'S WORLD MARKET SHARE AND HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
World import/export share of target, t-1 # Log trade, sender to target, t-1	0.0190**	0.0037
	(0.0081)	(0.0060)
Energy # World import/export share of target, t-1 # Log trade, sender to target, t-1	-0.0347	-0.0182
	(0.0274)	(0.0129)
Non-ferrous metal # World import/export share of target, t-1 # Log trade, sender to target, t-1	-0.0249***	-0.0226*
	(0.0075)	(0.0116)
Chemicals # World import/export share of target, t-1 # Log trade, sender to target, t-1	0.0128	0.0112
	(0.0143)	(0.0140)
Electronics # World import/export share of target, t-1 # Log trade, sender to target, t-1	-0.0194***	-0.0120
	(0.0064)	(0.0114)
Nuclear # World import/export share of target, t-1 # Log trade, sender to target, t-1	-0.0421	-0.0006
	(0.0314)	(0.0316)
Armaments # World import/export share of target, t-1 # Log trade, sender to target, t-1	-0.0199	-0.0007
	(0.0168)	(0.0188)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.32	0.41
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). In column (1) the regression includes the variable "World import share of target, t-1" and its interactions, in column (2) the regression includes the variable "World export share of target, t-1" and its interactions. Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election

TABLE 3. 10: UBIQUITY OF EXPORTERS/IMPORTERS AND HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	0.0017	-0.0006
	(0.0016)	(0.0014)
Energy # Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	0.0033	0.0023
	(0.0024)	(0.0032)
Non-ferrous metal # Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	-0.0029	0.0064
	(0.0026)	(0.0041)
Chemicals # Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	-0.0019	-0.0203***
	(0.0023)	(0.0063)
Electronics # Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	0.0045**	0.0017
	(0.0021)	(0.0017)
Nuclear # Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	-0.0088	0.0048
	(0.0059)	(0.0234)
Armaments # Ubiquity of importers/exporters, t-1 # Log trade, sender to target, t-1	0.0015	-0.0031
	(0.0023)	(0.0043)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.32	0.42
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). In column (1) the regression includes the variable "Ubiquity of importers, t-1" and its interactions, in column (2) the regression includes the variable "Ubiquity of exporters, t-1" and its interactions. Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in

6.3 Rivalry

A sanction may be shunned if strategic market share considerations are at play with respect to the role of the sanction-sending country in a given product market. Countries may be less incentivized to initiate a sanction when the target country is an important buyer or supplier of a product for which the sanctioning state competes fiercely, either as an exporter or as an importer. The degree of *rivalry* an exporter is exposed to in a market for a strategic good may be crucial for the decision to sanction. On the other hand, if a strategic good is strongly contested by many potential buyers as is the case with oil, then a potential sanction may not be initiated against the country which delivers the contested good.

To measure rivalry in a product market, I employ a direct market-share instability measure borrowing from Sakakibara and Porter (2001). This measure has its theoretical underpinnings in Stigler (1964) and Allen (1976) who observe that instability in market positions is a sign of active competition whereas stable market share may be indicative of oligopolistic collusion.

Following Sakakibara and Porter (2001), I compute market-share instability from the sum of individual market-share fluctuations of each exporter and importer between t-6 and t-1. The measure for a country exporting a product *k* can be expressed as follows:

$$MSI^{ex}_{ik,\Delta t-5} = \frac{\sum_{i=1}^{n}\sum_{t=1}^{5}\left|\frac{exports_{ikt}}{exports_{kt}} - \frac{exports_{ikt-1}}{exports_{kt-1}}\right|}{5n}$$

where $exports_{ikt}$ are exports of a potential sanction sender *i* of product *k* in year *t*, $exports_{kt}$ are the total world exports of product *k* in year *t* and *n* is the total number of exporters. The market-share instability measure for a country importing a product k is constructed in an analogous manner.

The following specifications test if market share instability has an impact on the probability of sanction:

$$
\begin{aligned}
Pr(HR\_sanction_{ijt}) &= \beta_0 + \beta_1 strategic_{ijk} + \beta_2 trade_{ijk,t-1} \\
&+ \beta_3 MSI^{trade}_{ik,\Delta t-5} + \beta_4\, strategic_{ijk} * trade_{ijk,t-1} \\
&+ \beta_5\, strategic_{ijk} * MSI^{trade}_{ik,\Delta t-5} + \beta_6 trade_{ijk,t-1} \\
&* MSI^{trade}_{ik,\Delta t-5} \\
&+ \beta_7 strategic_{ijk} * trade_{ijk,t-1} * MSI^{trade}_{ik,\Delta t-5} + X_{ij(t-1)} \\
&+ Q_{it-1} + Z_{jt-1} + \alpha_i + \tau_t + \varepsilon_{ijt}
\end{aligned}
\tag{3.5}
$$

The variable $trade_{ijk,t-1}$ stands in for either $exports_{ijk,t-1}$ or $imports_{ijk,t-1}$, i.e., for bilateral export and import flows at the product level and the variable $MSI^{trade}_{ik,\Delta t-5}$ stands in for either $MSI^{ex}_{ik,\Delta t-5}$ or $MSI^{im}_{ik,\Delta t-5}$. The results from estimations of the moderating effect of market-share instability on impact of strategic trade on the probability of human rights sanction are shown in Table 3.11. Interestingly, neither export nor import share instability in strategic products has any impact on the decision to initiate a sanction case. Where export share instability counts is in the exports of non-strategic goods. The higher the market share instability an exporter faces in the markets for non-strategic goods, the lower the probability to initiate a sanction against those countries to which the exporter delivers those goods.

TABLE 3. 11: MARKET SHARE INSTABILITY AND HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	-0.1205**	0.0031
	(0.0582)	(0.0669)
Energy # Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	0.0309	-0.1197
	(0.1117)	(0.4034)
Non-ferrous metal # Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	0.0224	-0.0654
	(0.1507)	(0.3087)
Chemicals # Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	0.1966	0.2436
	(0.1354)	(0.2776)
Electronics # Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	0.0932	0.4235
	(0.1267)	(0.2702)
Nuclear # Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	0.3628	0.0050
	(0.2821)	(0.1902)
Armaments # Export/import share instability, sender, last 5 ys # Log trade, sender to target, t-1	0.1179	-0.0515
	(0.1444)	(0.3862)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.32	0.41
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). In column (1) the regression includes the variable "Export share instability, sender, last 5 ys" and its interactions, in column (2) the regression includes the variable "Import share instability, sender, last 5 ys" and its interactions. Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election

6.4 Non-economic factors

Conditional on economic and political fundamentals I test if human rights-concerned countries sanction less often countries that they are geopolitically aligned with. Human rights-concerned countries are interested in preventing a potential destabilization of countries with which they collaborate in international affairs or are of military importance. China's imposition of (and follow-through with) human rights sanctions on North Korea may destabilize the authoritarian regime of its neighbor and lead to a civil conflict and North Korean migrant inflows to China, a situation China would like to prevent (Albert, 2017). On the other hand, delivery of strategic goods by human-rights concerned countries to human rights-abusing countries can also serve the strategy of strengthening ties between geopolitical allies. US arms deliveries to Saudi Arabia, an American-friendly oil producer, allows the US a sustained access to oil, a product that has been tied to American national security (Jones, 2012). In sum, human rights-promoting states have numerous incentives to abstain from sanctioning aligned countries since a sanction imposition may come with high economic, political and security costs.

To measure whether countries that are geopolitically aligned with a sanctioning country have lower probability to be sanctioned, I use bilateral voting similarity in the UN General Assembly as a proxy for geopolitical alignment between two countries. This variable is defined as the share of votes in which the human rights-concerned country and the human rights abusing country vote identically, i.e., both voting yes, no or abstaining. This measure has been widely used to approximate bilateral geopolitical closeness (Barro and Lee, 2005; Dreher et. al., 2008; Qian and Yanagizawa-Drott, 2009, Qian and Yanagizawa-Drott, 2017).

Table 3.12 shows the results from a regression of the main specification (3.1) in which the key explanatory variables of interest have been interacted with UN voting similarity. The interactions of UN voting similarity with imports and exports of strategic and non-strategic goods are not statistically significant at conventional levels. Thus, there is no evidence that geopolitical ties between

sanction-sending and human rights-violating countries moderate the influence of strategic trade on the probability of a human rights sanction.

TABLE 3. 12: GEOPOLITICAL MOTIVATION OF HUMAN RIGHTS SANCTIONS

	Exports	Imports
	(1)	(2)
UN voting similarity, t-1 # Log trade, sender to target, t-1	-0.0003	-0.0047
	(0.0023)	(0.0032)
Energy # UN voting similarity, t-1 # Log trade, sender to target, t-1	-0.0002	0.0059
	(0.0017)	(0.0045)
Non-ferrous metal # UN voting similarity, t-1 # Log trade, sender to target, t-1	0.0012	0.0033
	(0.0021)	(0.0033)
Chemicals # UN voting similarity, t-1 # Log trade, sender to target, t-1	-0.0027	-0.0004
	(0.0032)	(0.0036)
Electronics # UN voting similarity, t-1 # Log trade, sender to target, t-1	-0.0012	-0.0018
	(0.0013)	(0.0013)
Nuclear # UN voting similarity, t-1 # Log trade, sender to target, t-1	-0.0012	-0.0067
	(0.0034)	(0.0066)
Armaments # UN voting similarity, t-1 # Log trade, sender to target, t-1	0.0014	0.0037
	(0.0022)	(0.0035)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.40	0.53
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7. Robustness checks

In this section, I perform robustness checks of my results with respect to different modifications of the empirical strategy.

7.1 Product composition of the trade relationship

First, I control for the different magnitudes of trade flows between country pairs. Instead of using logged bilateral trade flows, I construct a measure of the relative composition of trade flows between two countries. This measure is a ratio of the bilateral trade flows in a given product over the total bilateral trade flows:

$$\frac{exports_{ijk,t-1}}{exports_{ij,t-1}}$$

$$\frac{imports_{ijk,t-1}}{imports_{ij,t-1}}$$

Rerunning specification (3.1) with the ratio measures instead of logged bilateral trade flows delivers qualitatively similar results. The higher the share of chemicals in the bilateral exports of a human rights-promoting country to a human rights-violating country, the higher the probability to initiate a sanction (Table 3.13). A higher share of nuclear products relative to the total bilateral exports decreases the sanction probability. The more energy products a potential sanction sender imports from a potential target, relative to the total bilateral imports, the lower the likelihood for a human rights sanction. A negative effect on sanction probability is observed also for imports of chemicals, electronics, nuclear products and armaments.

TABLE 3. 13: PRODUCT COMPOSITION OF THE BILATERAL TRADE RELATIONSHIP

	Exports	Imports
	(1)	(2)
Relative importance of a good within sender's exports to/imports from target, t-1	0.0014	0.0169***
	(0.0117)	(0.0062)
Energy # Relative importance of a good within sender's exports to/imports from target, t-1	0.0303	-0.0383***
	(0.0232)	(0.0135)
Non-ferrous metal # Relative importance of a good within sender's exports to/imports from target, t-1	-0.0451	0.0055
	(0.0293)	(0.0224)
Chemicals # Relative importance of a good within sender's exports to/imports from target, t-1	0.1146**	-0.0312*
	(0.0545)	(0.0172)
Electronics # Relative importance of a good within sender's exports to/imports from target, t-1	0.0017	-0.0501***
	(0.0252)	(0.0082)
Nuclear # Relative importance of a good within sender's exports to/imports from target, t-1	-0.3303***	-0.0382*
	(0.0686)	(0.0224)
Armaments # Relative importance of a good within sender's exports to/imports from target, t-1	-0.0290	-0.0556***
	(0.0224)	(0.0183)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.27	0.29
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. In column (1) the regression includes the variable "Relative importance of a good within sender's exports to target, t-1" and its interactions, in column (2) the regression includes the variable "Relative importance of a good within sender's imports from target, t-1" and its interactions. Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy

7.2 Change of the relationship over time

Next, I check whether my results are driven by a specific period in the sample such as the Cold War period or the post-Cold War era. In order to do this, I add interaction terms between the logged bilateral trade flows and decade dummies and present the results in Table 3.14. Exports of chemicals and electronics increase the sanction probability throughout the whole sample period. Exports of nuclear products and armaments have a negative impact on the probability of sanction and are statistically significant in all decades but the 1980s. Imports of energy products have a negative, statistically significant effect on the likelihood of a sanction initiation for all decades but the 1970s. Imports of non-ferrous metals and chemicals have a negative impact on sanction probability in the 1980s whereas nuclear products have a negative effect on sanction probability in the 1990s. Thus, whereas the effect of exports on sanction probability does not seem to be driven by a particular time period, the effect of chemicals and non-ferrous metals imports is limited to the Cold War period, whereas the effect of nuclear imports is only evident after the end of the Cold War.

TABLE 3. 14: STRATEGIC PRODUCTS BIAS IN HUMAN RIGHTS SANCTIONS, BY DECADE

	Exports	Imports
	(1)	(2)
log trade, sender to target, t-1, 1970s	0.0008	-0.0007
	(0.0006)	(0.0004)
Energy # log trade, sender to target, t-1, 1970s	0.0003	-0.0023
	(0.0005)	(0.0015)
Non-ferrous metal # log trade, sender to target, t-1, 1970s	0.0002	-0.0015
	(0.0005)	(0.0012)
Chemicals # log trade, sender to target, t-1, 1970s	0.0021***	0.0010
	(0.0007)	(0.0007)
Electronics # log trade, sender to target, t-1, 1970s	0.0009***	-0.0003
	(0.0003)	(0.0005)
Nuclear # log trade, sender to target, t-1, 1970s	-0.0028**	-0.0034
	(0.0012)	(0.0031)
Armaments # log trade, sender to target, t-1, 1970s	-0.0013**	-0.0003
	(0.0006)	(0.0011)
log trade, sender to target, t-1, 1980s	0.0019***	0.0050***
	(0.0007)	(0.0015)

Energy # log trade, sender to target, t-1, 1980s	0.0001	-0.0034***
	(0.0004)	(0.0013)
Non-ferrous metal # log trade, sender to target, t-1, 1980s	0.0007	-0.0021**
	(0.0006)	(0.0009)
Chemicals # log trade, sender to target, t-1, 1980s	0.0018***	0.0022***
	(0.0006)	(0.0007)
Electronics # log trade, sender to target, t-1, 1980s	0.0005*	-0.0006
	(0.0003)	(0.0004)
Nuclear # log trade, sender to target, t-1, 1980s	-0.0012	-0.0022
	(0.0012)	(0.0018)
Armaments # log trade, sender to target, t-1, 1980s	-0.0010	-0.0005
	(0.0007)	(0.0012)
log trade, sender to target, t-1, 1990s	0.0012	0.0015
	(0.0008)	(0.0010)
Energy # log trade, sender to target, t-1, 1990s	0.0000	-0.0027**
	(0.0004)	(0.0012)
Non-ferrous metal # log trade, sender to target, t-1, 1990s	0.0002	-0.0009
	(0.0005)	(0.0009)
Chemicals # log trade, sender to target, t-1, 1990s	0.0016***	0.0015*
	(0.0006)	(0.0008)
Electronics # log trade, sender to target, t-1, 1990s	0.0006**	-0.0005
	(0.0003)	(0.0004)
Nuclear # log trade, sender to target, t-1, 1990s	-0.0025**	-0.0031**
	(0.0011)	(0.0015)
Armaments # log trade, sender to target, t-1, 1990s	-0.0013**	-0.0008
	(0.0006)	(0.0011)
log trade, sender to target, t-1, 2000s	-0.0011	-0.0013***
	(0.0007)	(0.0003)
Energy # log trade, sender to target, t-1, 2000s	-0.0002	-0.0020**
	(0.0004)	(0.0010)
Non-ferrous metal # log trade, sender to target, t-1, 2000s	-0.0006	-0.0014
	(0.0004)	(0.0011)
Chemicals # log trade, sender to target, t-1, 2000s	0.0015**	0.0003
	(0.0006)	(0.0006)
Electronics # log trade, sender to target, t-1, 2000s	0.0009**	-0.0001
	(0.0004)	(0.0004)
Nuclear # log trade, sender to target, t-1, 2000s	-0.0034***	-0.0033
	(0.0010)	(0.0021)
Armaments # log trade, sender to target, t-1, 2000s	-0.0014**	-0.0007
	(0.0006)	(0.0010)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.32	0.43
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports (column (1)) and log imports (column (2)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7.3 Sanctions imposition

Next, I restrict the definition of a sanction case to sanctions imposition only. The results from rerunning specification (3.1) with the modified dependent variable are shown in Table 3.15. Whereas the coefficients for exports remain qualitatively similar to the benchmark specification, there are some differences in the results for imports as compared with the benchmark. Specifically, the probability for sanction imposition is negatively affected by imports of electronics and armaments. Neither energy imports nor chemicals and nuclear goods imports, which were significant determinants of the decision to initiate a sanction case play any role for the decision to impose a sanction.

TABLE 3. 15: STRATEGIC PRODUCTS BIAS IN HUMAN RIGHTS SANCTIONS IMPOSITION

	Exports	Imports
	(1)	(2)
log trade, sender to target, t-1	0.0003	0.0007*
	(0.0003)	(0.0004)
Energy # log trade, sender to target, t-1	0.0003	-0.0013
	(0.0003)	(0.0008)
Non-ferrous metal # log trade, sender to target, t-1	0.0001	-0.0006
	(0.0004)	(0.0006)
Chemicals # log trade, sender to target, t-1	0.0010**	0.0004
	(0.0005)	(0.0006)
Electronics # log trade, sender to target, t-1	0.0004**	-0.0005**
	(0.0002)	(0.0002)
Nuclear # log trade, sender to target, t-1	-0.0017*	-0.0017
	(0.0009)	(0.0011)
Armaments # log trade, sender to target, t-1	-0.0010**	-0.0014*
	(0.0004)	(0.0008)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.28	0.36
N	1,659,158	645,267

Notes: Dependent variable: imposition of a human rights sanction at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country. * $p < 0.10$, ** $p < 0.05$,

7.4 Sanction type

Finally, I account for the different types of sanctions. Whereas the majority of the sanction cases provided for some kind of trade restriction – imports or exports restrictions, embargo, blockade or suspension of economic agreement – some featured only measures which may only indirectly affect trade. Among them are sanction types such as asset freezing, travel bans or termination of foreign aid. I exclude these non-trade-type sanctions and rerun specification (3.1). The

results in Table 3.16 show little change for exports. With the exception of nuclear product exports which are not statistically significant any more, all other strategic products keep their sign and statistical significance. On the imports side, energy products still have a negative impact on the decision to initiate a sanction, whereas chemicals and nuclear products do not contribute in a statistically significant way to this decision any more. However, non-ferrous metals become statistically significant and have a negative impact on sanction probability.

TABLE 3. 16: TRADE SANCTIONS

	Exports	Imports
	(1)	(2)
log trade, sender to target, t-1	0.0010***	0.0018***
	(0.0003)	(0.0006)
Energy # log trade, sender to target, t-1	-0.0001	-0.0027**
	(0.0003)	(0.0011)
Non-ferrous metal # log trade, sender to target, t-1	0.0008**	-0.0023**
	(0.0004)	(0.0009)
Chemicals # log trade, sender to target, t-1	0.0017***	0.0011
	(0.0005)	(0.0007)
Electronics # log trade, sender to target, t-1	0.0005**	0.0000
	(0.0002)	(0.0003)
Nuclear # log trade, sender to target, t-1	-0.0003	-0.0021
	(0.0008)	(0.0013)
Armaments # log trade, sender to target, t-1	-0.0007*	-0.0004
	(0.0004)	(0.0010)
Non-interacted main explanatory variables	yes	yes
Control variables	yes	yes
Sender FE	yes	yes
Year dummies	yes	yes
R-squared	0.19	0.37
N	1,659,158	645,267

Notes: Dependent variable: initiation of a human rights trade sanction case at the country-pair-year level. The variable "log trade" is a placeholder for log exports of sender to target (column (1)) and log imports of sender from target (column (2)). Standard errors are in parentheses. Standard errors are clustered at sender-year level. All regression include a full set of uninteracted variables from which the interaction terms are constructed and the following control variables: contiguity, common colonizer, number of members of GATT/WTO in the country pair, trade dispute within the country pair, election year in sender country, democracy level in sender country, democracy level in target country, human rights abuse index in target country, political and ethnic murders in target country.

8. Conclusion

There has been a long-standing debate as to what extent economic relations between countries influence the decision to take measures against human-rights violating countries (Tomasevski, 1997; Alesina and Dollar, 2000, Neumayer, 2003; Barratt, 2008; Nielsen, 2014). This paper contributes to this debate in the following way. I find that human rights-concerned countries impose sanction for human rights violations, but do so selectively. The countries that disregard human rights are sanctioned less often if they engage in trade in strategic goods, such as natural resources, armaments or high tech goods with the human rights-concerned countries. Variations in the degree to which countries that violate human rights import or export strategic goods explain the differing treatment with sanctions that result from comparable human rights violations.

The main channel that drives the differential treatment of countries trading in strategic goods are trade specialization patterns. In particular, I find that strategic trade has a stronger effect on the use of sanctions for sanction senders that are more dependent on the potential target or that find it harder to substitute the trade relationship with the potential target. The logic behind these findings is that not every trade flow disruption has the same opportunity costs – no matter whether it is in strategic or non-strategic goods. I also test for the moderating effect of close political ties between human rights-promoting and human rights-violating countries but do not find any effect.

My findings offer new insights on the existing body of contradictory findings linking trade to sanctions. If countries primarily sanction human rights abusers with which they trade in particular strategic commodities, the effects of trade on sanction initiation may be underestimated in the existing literature that only examined aggregated trade.

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Chapter 4: Sanctions effect persistence

Using a country-pair panel of economic sanctions from 1965 to 2005, I estimate the effect of sanctions imposition on bilateral trade flows. I explore both trade dynamics during the sanction period, as well as the long-run effect on trade after the sanction is lifted. I find that trade flows between sanction sender and target drop substantially after the sanction starts. These effects are long-lived, and persist well after the sanction has ended. There is no negative effect of the sanction on target's total trade. The sanctioned country is able to divert trade towards non-sanctioning competitors of the sanction sender.

JEL-Codes: F51, F13, F14, D74, N40

Keywords: sanctions, bilateral trade, persistence, trade diversion

1. Introduction

When a country imposes a sanction in the form of a bilateral trade restriction, does it harm its long-term relationship with the sanctioned country? Moreover, does the sanction target turn to other trade partners that seize the opportunity to expand their trade relations with the target? In 2016, Iran's sanction regime was lifted. Did trade between Iran and the countries that had sanctioned it pick up swiftly? In 2018 the US threatened to reinstall its sanction regime. If the US follows up on its threat, will Iran intensify its trade relations with European non-sanctioning countries?

In this paper, I explore the effects of economic sanctions imposition. My focus is on trade outcomes, particularly bilateral trade between the sender and the target and between the target and non-sanctioning countries. The economic sanctions that I use are restrictions on trade of different duration – from 1 to 13 years.

They are imposed for various political and economic reasons, from arms proliferation and human rights violations to involvement in unfair trade practices. To assess the effects of these sanctions on bilateral trade, I conduct a synthetic control analysis.

For each sanction case in my data, I construct a synthetic control sanction target from a donor pool of trade partners of the sanction sender that are similar to the country being sanctioned. I then evaluate the quality of each synthetic control by observing how well each one approximates the sanction target in the pre-sanction period. The synthetic controls that do not show a good fit are removed from the subsequent estimation. I aggregate all of the remaining, well-performing estimates to a single estimate for each group of sanction cases of equal duration.

I find that sanctions depress bilateral trade flows for the time period the sanction is in place. Moreover, seven years after a sanction has been lifted, trade between sender and target has not reversed to pre-sanction levels. Examining the total trade flows of the target, I find no evidence for any impact of the sanction on sanctioned country's total trade. I then investigate potential trade diversion by exploring the effects of the sanction on target's trade with non-sanctioning countries. The target strengthens its trade relations during and after the sanction with countries that are geopolitically close to the sanction sender and not to the target. This opportunistic behavior of countries not involved in a sanction case is confirmed when testing for change in trade relations during and after sanction between the target and countries with export structure similar to the one of the sanction sender. I find that target's trade is diverted towards competitors of the sanctioning country.

The decline and the non-reversal of bilateral trade between sanction sender and target may point to the deterioration of "trading capital" as suggested by Head et. al. (2010), Beestermöller and Rauch (2018) and Gokmen et. al (2018). The sanction years can severely impair the political and economic relations between the sender and the target. The persistent underperformance of bilateral trade relative to the pre-sanction period even after the sanction has ended implies the destruction of trading capital and the accumulation of trading capital with

countries not involved in the sanction. To the best of my knowledge, the persistence of sanction effect on trade even after the end of the sanction has not been previously explored. My paper also adds to the literature that shows that recovery from trade interruption is a very long process (Felbermayr and Gröschl, 2014; Nitsch and Wolf, 2013).

Changes in the political relations between two countries can strongly influence trade relations. Interstate frictions negatively affect economic relations – differences in political and ideological affinity reduce trade (Mityakov et. al., 2013) as do separate acts of political animosity between countries (Michaels and Zhi, 2010; Fuchs and Klann, 2013; Fisman, et. al. 2014; Heilmann, 2016). These studies focus either on a single market or on the bilateral trade of a single country. I measure the effect of foreign policy conflict-induced trade restrictions on bilateral and third-party trade for a sample of 22 sanction senders and 44 sanction targets for a total of 150 sanction cases from 1965 to 2005.

Much of the research contributions on the relationship between sanctions and trade come from political science. Several studies explore the impact of sanctions on trade (Evenett, 2002; Hufbauer et al., 2009) whereas another strand of literature examines the effect of third-party trade with the target for the success of US sanctions (Caruso, 2003; Kaempfer and Ross, 2004; Yang et. al., 2004; Early, 2009). The focus of these studies is the success of the sanctions in achieving their desired objectives, whereas I explore the trade-depressing and trade-diverting effects of sanctions. Moreover, these studies use cross-sectional or panel approaches that suffer from unobserved confounding factors. By employing the synthetic control method, I construct data-driven counterfactuals and thus overcome the omitted variables bias.

My paper also adds to the trade diversion literature. While the majority of the contributions focus on the trade-creating and trade-diverting effects of preferential trade agreements, my study relates most closely to those papers that examine trade diversion in response to antidumping measures (Prusa, 1997, 2001; Bown and Crowley 2006, 2007, 2010; Shen and Fu, 2014). Antidumping duties decrease trade with countries affected by the duty and increase trade of the

antidumping target with third parties. Considering trade sanction as a tax on trade, I examine trade diversion during and after sanctions. Who do target countries turn to trade with? Contrary to the findings of the political economy literature that political regime proximity increases trade (Morrow et. al., 1998; Mansfield et. al. 2000; Long and Leeds, 2006; Aidt and Gassebner, 2010), I find that sanction targets do not increase trade with their geopolitical allies but turn to trade with those allies of the sanction sender that have a similar export structure as the sender.

While a large number of papers have employed the synthetic control approach on individual case studies since its inception with the study by Abadie et. al. (2003), there exists a small but growing literature that applies synthetic control analysis on multiple treatments. Several studies explore the health and labor market outcomes of different policy changes for multiple treated units (Dube and Zipperer, 2015; Donohue et. al., 2017; Hall et. al., 2017; Lepine et. al., 2017), whereas Acemoglu et. al. (2016) assess the performance of politically connected firms. Methodically closest to my study are the contributions by Saia (2017) who explores the hypothetical change in trade flows had the UK joined the Eurozone, and Hall et. al. (2017) and Cavallo et. al. (2013) who deal with multiple recurring events.

The rest of the paper is organized in the following way. Section 2 presents theoretical considerations. Section 3 describes the synthetic control method and the data. Section 4 presents the main result of the synthetic control estimation of sanction on trade. Section 5 explores the trade diversion effects for the target, following the sanction. Section 6 concludes.

2. Theoretical considerations

What happens when a sanction in the form of trade restriction is introduced? Borrowing from Bown and Crowley (2007), in a world with three countries – sender (S), target (T) and non-sender (NS) – where trade is free, let S impose a sanction on imports from T. Conceptually, this can be thought of as an

imposition of a prohibitively high import tariff. How does that affect trade flows among the three countries? Assume a simplified setting where each country has one firm producing a single good and firms compete in quantities. Each good is produced with the same technology and the goods are substitutes. The marginal cost of production is increasing for each firm. Firms sell their good at home and in the two foreign markets. Each firm chooses a total output level and how much it well sell at home and abroad so that is maximizes its profits. In a free trade world, the firm's best response to the other firms' sales decisions is to allocate its sales across the three markets so that its net marginal revenue is the same in every market (a Cournot Nash equilibrium).

With three countries, three firms with increasing marginal costs in production and goods that are substitutes, an imposition of a sanction changes the free trade equilibrium in the following way: (1) Target T's exports to the sender S decrease, (2) Target T's exports to non-sender NS increase. These results depend on the assumption of increasing marginal costs of production. In particular, the trade diversion comes about because of the higher costs (due to the sanction imposition) of selling to the sender (S). In equilibrium, the net marginal revenues should be equal across the three markets, if they are not, then firms will divert their sales away from the market with the higher costs of selling to the other markets.

I now turn to empirically test the theoretical predictions.

3. Synthetic control method

I estimate the impact of sanctions on trade with the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al. 2010, 2015). This approach has the advantage of overcoming the problem of omitted variable bias and consistently defining a control group. The synthetic control method follows a data-driven approach in the selection of the control pool – it takes the weighted average of all available control units, whereby the weights are chosen to ensure that the synthetic control group resembles the treated units in the outcome variable and

in all explanatory variables in the pre-treatment period. This matching on prior outcomes allows to indirectly control for any unobserved characteristics. The treatment effect is calculated as the difference between the treatment and synthetic control unit in the post-treatment period.

I first define the number of treatment events. I consider the entire period for which sanction data is available – 1948-2005. Throughout this period there are 1092 sanction cases recorded. I focus on those sanctions that impose any type of trade restriction – export or import restriction, embargo or blockade – and exclude sanction cases that impose other types of restrictions, such as termination of foreign aid, asset freeze or travel ban. I am then left with 498 sanction cases. Some sanction senders sanction the same target several times. Hence, I restrict my sample in the following way. I select those sanction cases for which there was no sanction in the country-pair 15 years prior to the start of the sanction case and 7 years after the sanction case ended. Each sanction case then consists of a bilaterally sanction-free 15-year pre-sanction window, a sanction window equal to the sanction duration and a bilaterally sanction-free 7 year post-sanction window. These limitations decrease the pool of treatment events to 150 sanction cases (Table A4.1).

There is a trade-off between the number of sanction cases and the window length for each case. Shortening the pre- and post-treatment periods increases the number of sanction cases that I can include but decreases the estimation quality as less pre-treatment data is used to construct the synthetic control estimator. In contrast, increasing the pre- and post-treatment periods decreases the number of sanction cases included and may also reduce the credibility of the estimates. My main estimation uses a pre-treatment period of 15 years but estimations with 10 and 20 years of pre-treatment delivered qualitatively similar results.

In terms of predictive variables, I use a set of three theoretically motivated variables – log of GDP of the sanction target, log of distance between the sender and the target and previous bilateral export and import levels. The synthetic control method weights these pre-treatment characteristics such that the mean-squared prediction error of the pre-sanction trade flows (the outcome variable)

is minimized. Specifically, the method ensures that the average of the squared differences between the target's bilateral trade flows with the sanction sender and the synthetic control unit's bilateral trade flows with the sender during the 15-year pre-sanction period is minimized.

For each sanction target in my sample, I select similar countries to construct the donor pool. It is important to restrict the donor pool to only those units that are similar to the treatment units in order to avoid overfitting (Abadie et. al., 2015). I construct a measure of similarity for target countries in the following way. First, I calculate the average dollar value difference in bilateral exports and imports with the sanction sender over the pre-treatment period between the target country and every other country in the world. Second, I construct the by-year correlation of the bilateral exports and imports with the sender between the target and every other country in the world. With these two steps I make sure that two countries are similar not only in terms of bilateral trade levels with the sender but also in terms of how their bilateral trade flows with the sender evolve over time. Finally, I normalize the two measures and average them into a single measure of export or import similarity. I then select the top 20 most similar countries for each target country and assign them to the donor pool[11].[12]

Given that the pre-treatment period is long enough, the convex combination of counterfactual units will replicate any observed or unobservable determinants of bilateral trade flows for each country pair. If the synthetic control unit approximates the treated unit well enough in the pre-treatment period, then any subsequent difference between treated and control units will represent the effect of the sanction on bilateral trade flows.

I use the following data for the synthetic control analysis. Information on sanctions comes from the Threat and Imposition of Economic Sanctions dataset,

[11] Estimation results remain qualitatively similar when I choose the top 50 or the top 10 countries to be part of the donor pool.

[12] A different approach to ensure comparability of the donor pool to the treatment units is to restrict the pool according to similarity of the predictor variables in the pre-treatment period. To that end, I restrict the donor pool to countries that differ by not more than +/-100% in terms of GDP of the treated target and +/-100% in terms of distance of the treated target to the sender. Estimation results remain comparable to my main result.

by Morgan, Bapat and Kobayashi (2014). The data has been collected via human search in Lexis-Nexis, Facts on File, Keesing's Record of World Events, the New York Times index, and the London Times index. Sanctions are defined as actions a country undertakes to reduce or cut its economic ties with a target country, aiming thereby to change the target country's behavior or policies. The dataset covers 1092 sanction cases from 1945 to 2005 and is considered the most comprehensive source on sanctions occurrence available.

Data on bilateral trade flows comes from International Monetary Fund's Direction of Trade Statistics (DOTS). Lastly, data on current GDP is provided by the World Bank (World Development Indicators, WDI), whereas data on bilateral distance comes from the CEPII database. The sources of any additional data used in the paper are presented as I go along.

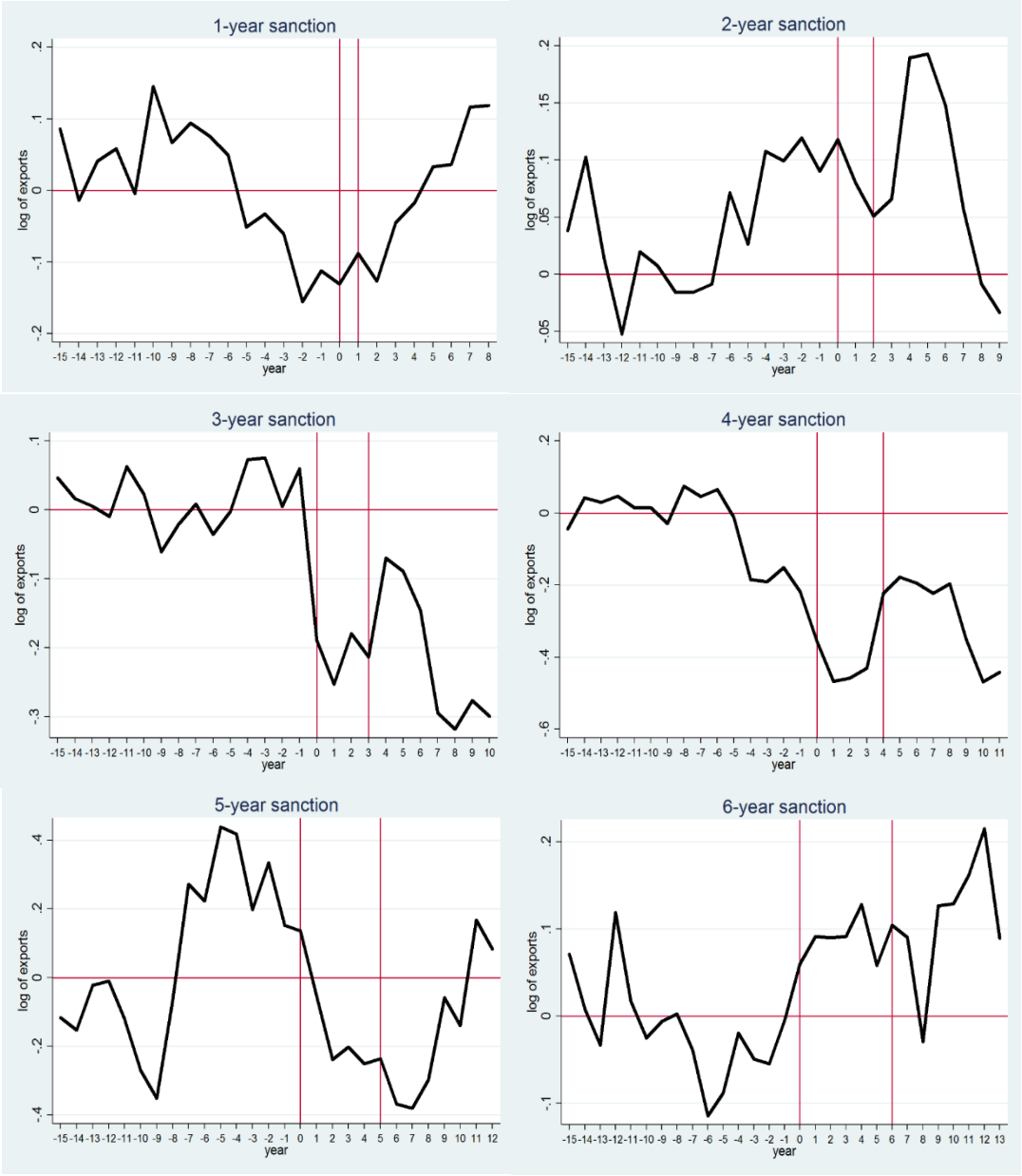
4. Synthetic control estimates of sanction effects on trade

I estimate the effect of a sanction on trade for every sanction case for which it was possible to construct a synthetic control. Going through the estimates one by one reveals that some of them are rather poor – for some sanction cases there are considerable gaps between the target country and the synthetic control group and hence the pre-treatment fit is bad. These cases also feature a high root mean square prediction error (RMSPE) for the pre-intervention period. I use this measure of the goodness of fit in the pre-period to take out poor synthetic control estimates.

I estimate the average effects on trade for all sanction cases and then choose RMSPE cutoffs of 0.4, 0.6, 0.8 and compute the average effects on trade for the remaining sanction cases. The results for the preferred cutoff of 0.6 are plotted in Figures 4.1 and 4.2, for exports and imports, respectively. The estimates for the other cutoffs as well as the estimates with all sanction cases are presented in the Appendix, Figures A4.1-A4.6. A comparison of the plots with different cutoffs underscores the need to screen out poorly fitting synthetic controls. When including all cases (i.e., the no-cutoff estimates), the pre-treatment fit of

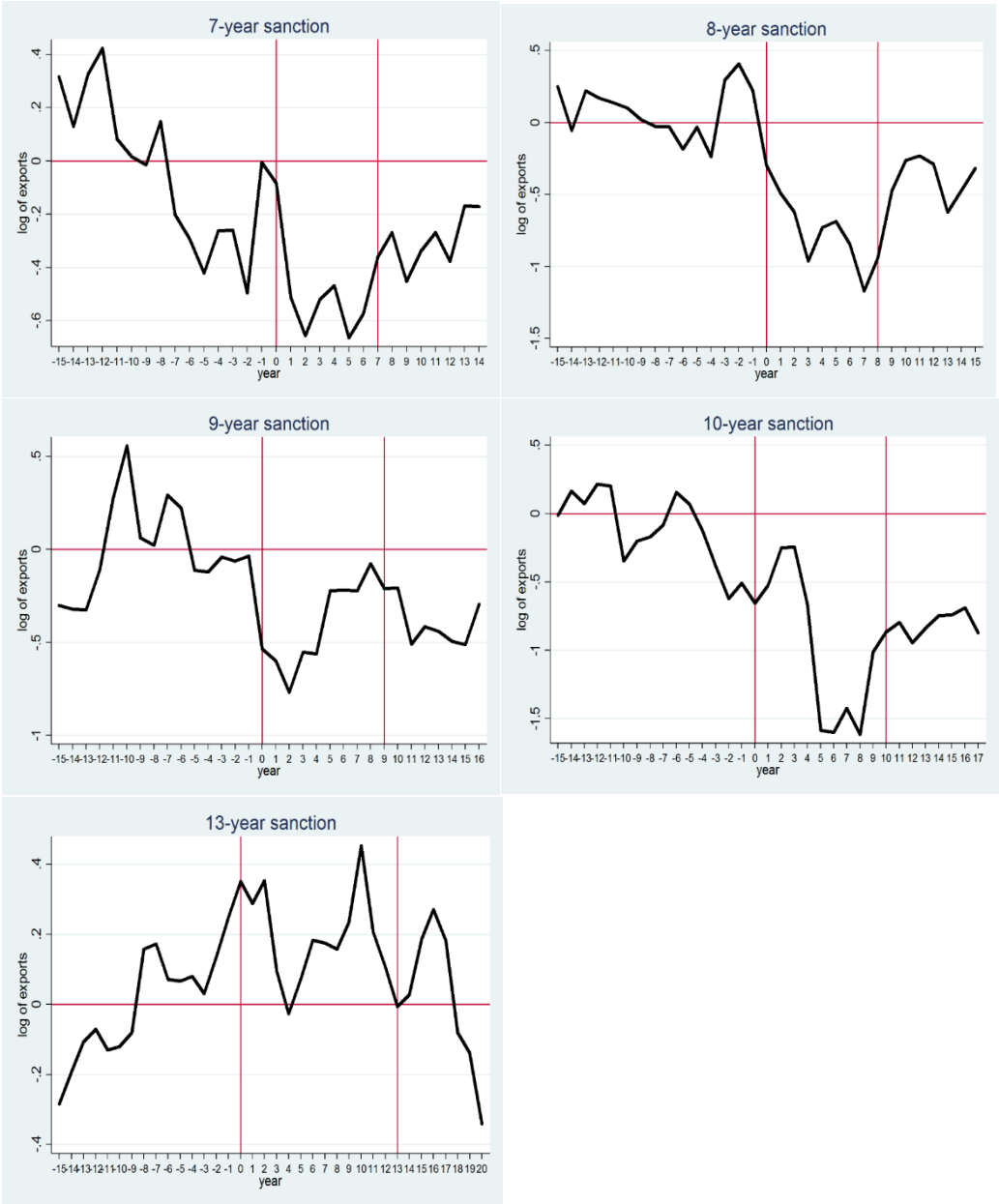
several sanction groups is particularly bad – 1-year, 2-year, 7-year and 9-year sanction cases feature either an extreme pre-intervention trend or a pre-intervention bias (Figures A4.5 and A4.6). Even if there is any effect of sanctions on trade, it is swamped by the pre-treatment bias. Limiting the calculation of the average effect to those sanction cases under a given threshold for the pre-sanction RMSPE moves the pre-treatment trend closer to zero.

FIGURE 4. 1: TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



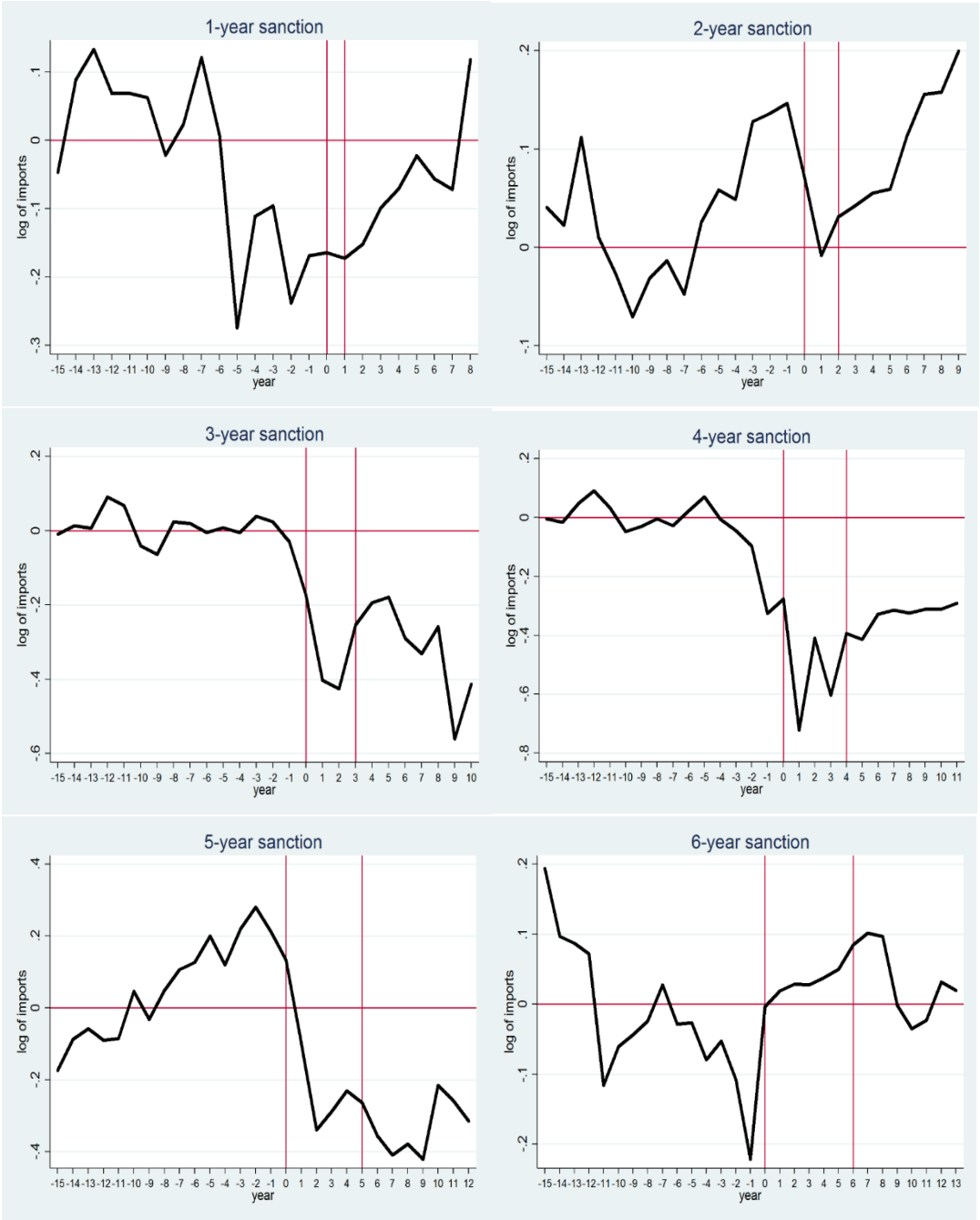
Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. Only sanction cases with pre-treatment RMSPE < 0.6 are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE 4. 1 (CONTINUED): TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



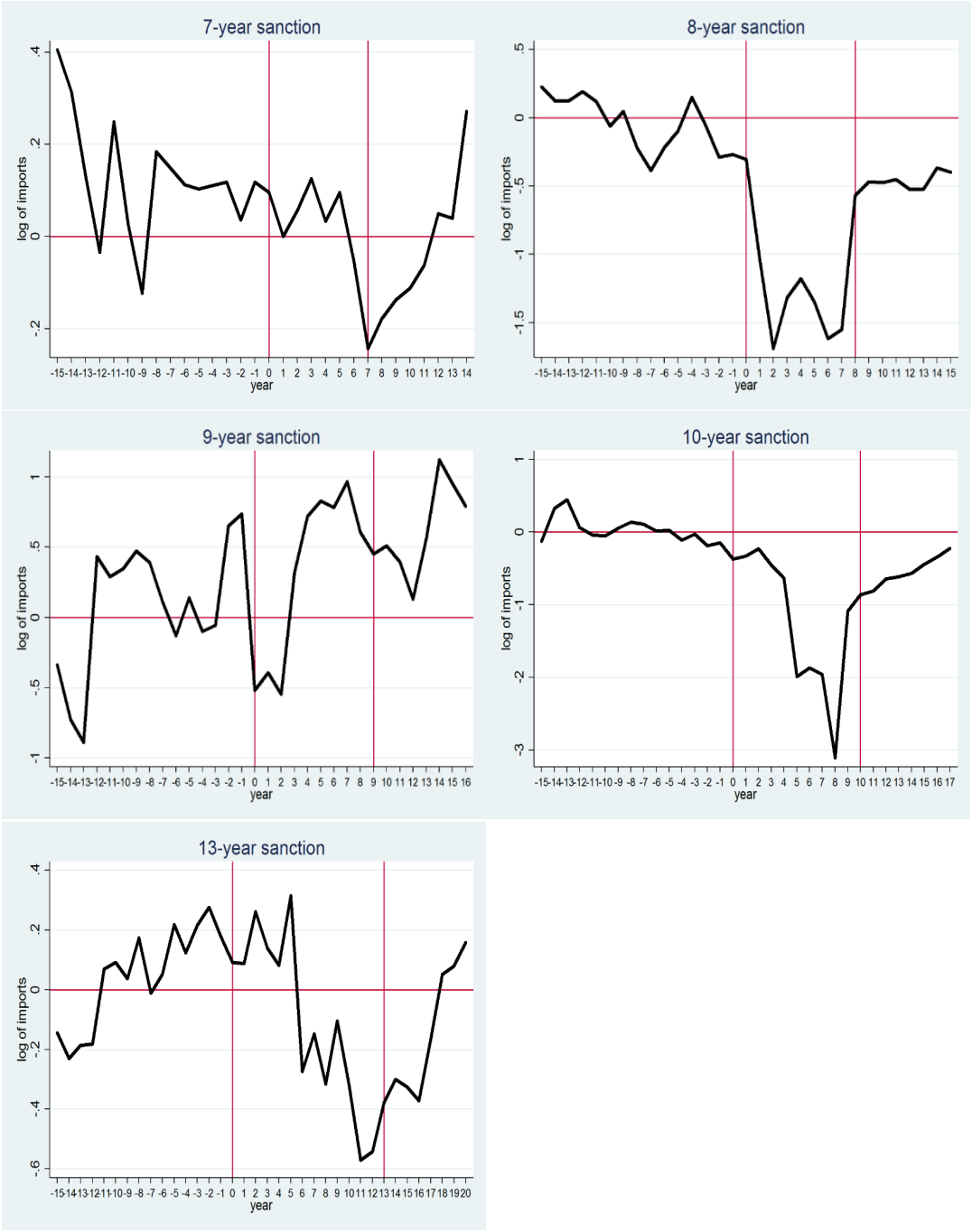
Notes: Figure continued from previous page.

FIGURE 4. 2: TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. Only sanction cases with pre-treatment RMSPE < 0.6 are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE 4. 2 (CONTINUED): TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



Notes: Figure continued from previous page.

There is a tradeoff in limiting the sanction cases according to their pre-treatment RMSPE – while the pre-treatment fit improves, the sample size is reduced. For example, whereas the full sample features 150 sanction cases, with a RMSPE cutoff of 0.4 we are left with a total of 96 sanction cases and some empty sanction groups (there are no 9-year sanctions left). I choose a cutoff of 0.6 which leaves me with 125 sanction cases and simultaneously remediates the strong pre-treatment trends and bias. Qualitatively, the results are reassuringly similar with the other cutoffs or when including all sanction cases.

Figures 4.1 and 4.2 show the by-year estimated treatment effects for exports and imports using the preferred screening method. The 11 panels in each figure correspond to 11 groups of sanctions of differing duration – 1 to 10 and 13 years long sanction cases. The thick black line in each panel represents the average trade gap between the treated countries and the synthetic control countries in the 15 pre-treatment, 1 to 13 treatment and 7 post-treatment years. Both estimates for imports and exports depict a common picture of the impact of sanctions on bilateral trade flows between a sanction sender and the target. There is a substantial persistent negative effect of sanctions on trade flows, which settles in shortly before a sanction is imposed, continues throughout the sanction duration and remains even after the sanction has been lifted. This effect is observed among the sanction groups of 1, 3, 4, 5, 7, 8, 9, 10 and 13 years of duration. The magnitude of the negative impact on trade flows increases with the increasing duration of the sanction. For 3-, 4-, 7-, 8-, 9-, and 10-year sanctions, trade flows do not seem to revert to their pre-sanction level even seven years after the sanction was lifted. After a sanction of 1, 5 or 13 years, a reversal to pre-sanction trade is observed only around three years after the end of the sanction. The few cases for which the average effect is not as strongly negative are sanction of 2 and 6 years and 2, 6, and 9 years of duration, for exports and imports, respectively. These results suggest that in the majority of cases sanctions generated a trade disruption and a persistent effect on the level of trade between the sender and the target.

5. Sanction's effect on target's trade

5.1 Effect on total trade

A sanction may have depressed a target's total trade or it may have diverted trade to other trade partners of the target. In the following, I provide an estimate of a sanction's effect on the total trade flows of a target.

In the previous section, I showed that trade flows between the target and the sender are lower during and after sanction. Thus, the explicit goal of the sanction to deprive a target of bilateral trade flows is fulfilled. However, if the target is able to divert its trade flows to other non-sanctioning countries, then the impact of the sanction would be moderated, or even fully counterbalanced. In this section, I explore whether the trade flows between the target and all of its trading partners are reduced during and after sanction.

If the aggregate exports or imports of the target decrease, then a sanction has caused trade destruction for the target. Otherwise, if trade does not change or if it increases during and after sanction, with contemporaneous trade decrease with the sender, then this is an indicator for trade diversion.

To test for the effect of a sanction on the total trade of the target I use a gravity-style approach and estimate a during- and post-sanction period dummy for all countries according to equation (4.1), controlling for GDP, population, country and time fixed effects. I run the regression for exports and imports separately and report results in Table 4.1.

$$T_{it} = \alpha_i + \beta_1 During_{it} + \beta_2 Post_{it} + \beta_3 \log GDP_{it} + \beta_4 \log Pop_{it} + year_t + \varepsilon_{it} \quad (4.1)$$

Estimates for both total exports and imports (columns (1) and (2)) show that the effects of the ongoing sanction as well as a seven-year post-sanction period

on total trade flows are not statistically different from zero. This remains true when looking at each year separately (columns (3) and (4)), where except for sanction years 11, 12, and 13 which have a positive effect on aggregate trade all the other coefficients are not statistically significant. This suggests that the decreased share of trade of the target with the sanction sender was compensated with a shift of imports and exports towards other countries. I next confirm this finding in a bilateral regression analysis in section 5.2 where I explicitly show the trade partners with which the target intensifies its trade relations.

TABLE 4. 1: THE EFFECTS OF A SANCTION ON TARGET'S TOTAL TRADE FLOWS

	ln exports	ln imports	ln exports	ln imports
	(1)	(2)	(3)	(4)
during sanction	-0.0001	-0.0005		
	(0.0009)	(0.0012)		
after sanction	-0.0002	-0.0019		
	(0.0022)	(0.0012)		
during sanction: t=1			-0.0002	-0.0013
			(0.0019)	(0.0014)
during sanction: t=2			-0.0004	-0.0021
			(0.0020)	(0.0019)
during sanction: t=3			-0.0001	-0.0006
			(0.0009)	(0.0016)
during sanction: t=4			-0.0002	0.0001
			(0.0011)	(0.0015)
during sanction: t=5			-0.0002	0.0006
			(0.0006)	(0.0009)
during sanction: t=6			-0.0001	0.0003
			(0.0006)	(0.0008)
during sanction: t=7			0.0007	0.0011
			(0.0006)	(0.0007)
during sanction: t=8			0.0005	0.0011
			(0.0007)	(0.0010)
during sanction: t=9			-0.0000	-0.0004
			(0.0010)	(0.0008)
during sanction: t=10			0.0007	-0.0001
			(0.0012)	(0.0009)
during sanction: t=11			0.0016***	0.0001
			(0.0005)	(0.0004)
during sanction: t=12			0.0014***	0.0007**
			(0.0004)	(0.0003)
during sanction: t=13			0.0011**	0.0007**
			(0.0004)	(0.0003)
after sanction: t=1			-0.0000	-0.0021
			(0.0022)	(0.0014)

after sanction: t=2			0.0001	-0.0023
			(0.0024)	(0.0015)
after sanction: t=3			-0.0003	-0.0025*
			(0.0026)	(0.0014)
after sanction: t=4			-0.0003	-0.0022
			(0.0024)	(0.0014)
after sanction: t=5			-0.0002	-0.0019
			(0.0024)	(0.0013)
after sanction: t=6			0.0001	-0.0015
			(0.0025)	(0.0015)
after sanction: t=7			-0.0000	-0.0017
			(0.0025)	(0.0013)
Control variables	yes	yes	yes	yes
Country FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
N	10,256	10,256	10,256	10,256
R-squared	0.99	0.99	0.99	0.99

Notes: The unit of observation is a country *i* in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in columns 1 and 3 is the natural log of imports from world; in columns 2 and 4 – natural log of exports from world. Control variables include log population and log GDP. Standard errors are clustered at the country level.
* p < 0.10, ** p < 0.05, *** p < 0.01

5.2 Trade with non-senders

In section 4 we observed that a sanction reduces bilateral trade between sender and target. Then, in section 5.1 we saw that the total trade of the target does not decrease. What happens to bilateral trade of the target with other countries that are not contemporaneously imposing a sanction?

In order to estimate the effects of sanctions on bilateral trade I need a benchmark for the trade that is expected if the sanction had not been imposed. A common practice in the empirical trade literature is to model expected bilateral trade with a specification derived from the gravity equation, which I am going to adopt here for my purposes.

My specification examines the effect of an imposed sanction on the bilateral exports and imports to and from non-sanctioning trading partners:

$$\ln Trade_{ijt} = \beta_1 During\_Nonseder_{ijt} + \beta_2 After\_Nonsender_{ijt} + X_{ijt}\gamma + \alpha_{ij} + \alpha_t + \varepsilon_{ijt} \quad (4.2)$$

where $Trade_{ijt}$ stands in for exports and imports, *i* indicates a potential sanction target country, *j* indicates a potential sanction sender country and *t* indicates year.

To measure the effect of years during and after sanction on trade with the rest of the countries not contemporaneously involved in sanctions against the target I include dummies – $During\_Nonseder_{ijt}$ and $After\_Nonsender_{ijt}$ – for target's trade with non-senders during sanction and seven years after a sanction is lifted. For illustration, consider the export and import restriction against Argentina imposed by USA, Great Britain and Germany in 1982 and lifted in 1989. The non-sender dummies during sanction or up to seven years after this sanction case turn on for the years 1982-1989 and 1990-1996, respectively, for every non-sanction-participating trade partner of Argentina during these periods. The β_1 and β_2 coefficients provide estimates of the effect of an imposed sanction on bilateral exports or imports of the sanctioned country with its non-sanctioning partners, during and after the sanction. A positive coefficient would imply that, on average, trade flows with non-sender countries increased relative to trade flows with sanction-sending countries.

X_{ijt} represents time-varying exporter-, importer or country-pair variables. These include indicators for membership in the GATT, common free trade agreement and common currency (from the CEPII database) and the logs of GDP and population for country *i* and *j* (World Development Indicators from the World Bank). α_{ij} are pair fixed effects used to account for the omitted variable bias stemming from omitted multilateral resistance terms (Anderson and van Wincoop, 2003). How much two countries would trade with each other depends on the barriers to trade between the two countries relative to the barriers between each country and the rest of the world. The excluded multilateral resistance terms depend on these bilateral trade barriers and vary over time. Thus, the country-pair fixed effects control for the bias stemming from the omission of the

multilateral resistance. These country-pair dummies allow for an estimation that relies on time-series variation in trade flows around country-pair averages and disposes of the cross-sectional variation between country pairs. As a result, I control for time-invariant differences between exporters or importers that may lead to the intensification or slow-down of trade during or immediately after a sanction period.

The year fixed effects α_t control for shocks common to all countries or trends that may confound the estimates – inflation trends, or oil shocks changing transport costs. Lastly, I cluster the standard errors ε_{ijt} at the country-pair level in order to allow for correlation between observations within the same pair.

I now turn to the results of my OLS estimation with country-pair fixed effects, which are reported in Table 4.2. Columns (1) and (2) show that sanctioned countries export disproportionately more to non-sanctioning countries during and after a sanction. This is also true for imports of sanction targets from non-senders, after a sanction is lifted. The estimated coefficients on exports during (0.054) and after (0.105) sanction imply that a target's exports to non-sanctioning countries is 5.4 percent greater than in no-sanction years and 10.5 percent greater than in years not immediately following the end of a sanction. For imports, the estimate is indicating 9.5 percent higher imports from non-senders relative to years that are not closely following the end of a sanction.

TABLE 4. 2: TARGET'S TRADE WITH NON-SENDERS DURING AND AFTER SANCTION

	ln exports	ln imports	ln exports	ln imports
	(1)	(2)	(3)	(4)
ongoing sanction for i where j is not the sender	0.0541***	0.0039		
	(0.0155)	(0.0180)		
7-year post-sanction period for i where j was not the sender	0.1050***	0.0951***		
	(0.0126)	(0.0158)		
during sanction: t=1			0.0743***	0.0264
			(0.0163)	(0.0212)
during sanction: t=2			0.0839***	0.05050**
			(0.0182)	(0.0231)
during sanction: t=3			0.0882***	0.0176
			(0.0232)	(0.0266)
during sanction: t=4			0.0724***	-0.0568*
			(0.0278)	(0.0303)
during sanction: t=5			0.1150***	0.0112
			(0.0343)	(0.0360)
during sanction: t=6			0.0717**	0.0016
			(0.0348)	(0.0402)
during sanction: t=7			-0.0311	-0.1580***
			(0.0595)	(0.0556)
during sanction: t=8			0.1540**	0.0018
			(0.0605)	(0.0646)
during sanction: t=9			0.1620**	0.1870**
			(0.0747)	(0.0809)
during sanction: t=10			-0.0276	0.1430*
			(0.0810)	(0.0851)
during sanction: t=11			-0.7290***	0.2700*
			(0.169)	(0.1570)
during sanction: t=12			-0.4290***	-0.0636
			(0.152)	(0.1110)
during sanction: t=13			-0.4050***	-0.2080
			(0.1510)	(0.1490)
after sanction: t=1			0.0829***	0.0420*
			(0.0168)	(0.0220)
after sanction: t=2			0.1220***	0.0596***
			(0.0169)	(0.0221)
after sanction: t=3			0.1310***	0.1180***
			(0.0168)	(0.0218)
after sanction: t=4			0.0907***	0.1100***
			(0.0170)	(0.0212)
after sanction: t=5			0.0797***	0.1010***
			(0.0168)	(0.0218)
after sanction: t=6			0.1150***	0.1040***
			(0.0168)	(0.0206)
after sanction: t=7			0.1110***	0.1330***
			(0.0162)	(0.0206)
Control variables	yes	yes	yes	yes
Country-pair FE	yes	yes	yes	yes

Year FE	yes	yes	yes	yes
N	788,053	729,759	788,053	729,759
R-squared	0.336	0.354	0.336	0.355

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in columns (1) and (3) is the natural log of exports of country i to country *j* in year *t*, in columns (2) and (4) – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for *i* and *j*, log GDP for *i* and *j*, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

To gain a better understanding of the evolution of the target-non-senders trade over time, I decompose the dummies $During\_Nonseder_{ijt}$ and $After\_Nonsender_{ijt}$ into indicator variables for each year during a sanction is in place and for the seven years after the sanction is lifted. The estimates for the newly created 13 dummies for trade during sanction and 7 dummies for trade post-sanction are reported in columns (3) and (4) of Table 4.2. From column (3) it stands out that exports to non-sanctioning countries are relatively higher than in years without a sanction, up until sanction year 9. Sanction year 10 is a turning point for a target's exports to non-senders. The year dummies become non-significant and then strongly and statistically significant negative for sanction years 11-13. This means that for the few quite long sanction cases, targets export less to non-senders than during non-sanction years. This may be due to the fact that for longer sanctions the sanction sender is over time able to persuade other countries to shun from trade with the target or it may signal the economic cost of a prolonged sanction which may negatively affect the competitiveness and the exports of a target over time. For imports (column (4)) the results are somewhat less stable, with most of the during-sanction-dummies insignificant, except for a positive impact for sanction years 2, 9, and 11 and a negative impact for sanction years 4 and 7. For the seven post-sanction years targets export and import more with non-senders than they do in other years not immediately following the end of a sanction. These results provide evidence for increased trade of sanction targets with non-sanctioning countries, during and after a sanction. In other words, countries under sanction, on average, redirect trade to the rest of the world. In the next section, I will explore which trade relations of the target intensify during and after sanctions.

To what extent are these implications of intensified trade relations with non-senders borne out among the different types of sanctions? Among the sanction cases in the sample there are two general types of sanctions – political ones, i.e., those initiated because of territorial dispute or military behavior of the target, aim of the sender to destabilize or politically influence the target or improve human rights conditions in the target, and the goal of the sender to prevent the target from acquiring weapons or strategic materials; and trade sanctions, i.e., those initiated because of particular trade practices of the target, such as protectionist measures, tariffs or other trade restrictions. While in the case of political sanctions the sanction sender may not be sufficiently economically engaged with the target and thus, may not value highly any loss of bilateral trade flows, for trade practices sanctions the particular reason for the sanction imposition is a trade-related one and a trade diversion by the target may be an unwanted side effect of the sanction for the sender.

In tables 4.3 and 4.4 I provide results after splitting the sample in political and trade sanctions. Both exports and imports of the target with non-sanctioning countries are negatively affected during an ongoing sanction whereas there is no effect after the sanction is removed (Table 4.3). On the other hand, trade sanctions drive up the target's exports and imports with countries not involved in the sanction. These intensified trade relations continue also after the sanction case ends (Table 4.4). As expected, politically motivated sanctions often pertain to issues related to widely held international norms and thus under international political pressure more countries may shun from trade with the target (Bapat and Morgan, 2009). In contrast, trade practices sanctions are often provoked by domestic economic concerns of the sanction sender and thus have a more bilateral character (Drury et. al., 2014).

TABLE 4. 3. TARGET'S TRADE WITH NON-SENDERS DURING AND AFTER POLITICAL SANCTION

	ln exports	ln imports	ln exports	ln imports
	(1)	(2)	(3)	(4)
ongoing political sanction for i where j is not the sender	-0.0549**	-0.0625**		
	(0.0235)	(0.0263)		
7-year post-political sanction period for i where j was not the sender	-0.0190	0.0002		
	(0.0175)	(0.0216)		
during sanction: t=1			-0.0544**	-0.0689**
			(0.0255)	(0.0310)
during sanction: t=2			-0.0448	-0.0151
			(0.0288)	(0.0330)
during sanction: t=3			-0.0992***	-0.1129***
			(0.0363)	(0.0413)
during sanction: t=4			-0.0329	-0.1559***
			(0.0393)	(0.0454)
during sanction: t=5			0.0205	0.0009
			(0.0409)	(0.0460)
during sanction: t=6			-0.0004	-0.0665
			(0.0402)	(0.0521)
during sanction: t=7			-0.0889	-0.1289
			(0.0708)	(0.0805)
during sanction: t=8			-0.0770	-0.0326
			(0.0753)	(0.1062)
during sanction: t=9			0.0286	0.0690
			(0.0922)	(0.1083)
during sanction: t=10			-0.1201	0.0728
			(0.0932)	(0.1045)
during sanction: t=11			-0.7602***	0.2494
			(0.1692)	(0.1577)
during sanction: t=12			-0.4622***	-0.0840
			(0.1522)	(0.1122)
during sanction: t=13			-0.4401***	-0.2293
			(0.1514)	(0.1502)
after sanction: t=1			-0.0727***	-0.0472
			(0.0255)	(0.0309)
after sanction: t=2			-0.0024	-0.0230
			(0.0250)	(0.0302)
after sanction: t=3			-0.0337	0.0311
			(0.0250)	(0.0306)
after sanction: t=4			-0.0410*	0.0335
			(0.0242)	(0.0299)
after sanction: t=5			-0.0462*	0.0285
			(0.0238)	(0.0295)
after sanction: t=6			0.0058	0.00159
			(0.0243)	(0.0290)
after sanction: t=7			-0.0048	0.0280
			(0.0234)	(0.0287)
Control variables	yes	yes	yes	yes
Country-pair FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

N	788,053	729,759	788,053	729,759
R-squared	0.336	0.354	0.336	0.354

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in columns (1) and (3) is the natural log of exports of country i to country *j* in year *t*, in columns (2) and (4) – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for *i* and *j*, log GDP for *i* and *j*, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

TABLE 4. 4: TARGET'S TRADE WITH NON-SENDERS DURING AND AFTER TRADE SANCTION

	ln exports	ln imports	ln exports	ln imports
	(1)	(2)	(3)	(4)
ongoing trade sanction for i	0.1715***	0.0600**		
where j is not the sender	(0.0189)	(0.0248)		
7-year post-trade sanction period	0.2074***	0.1594***		
for i where j was not the sender	(0.0155)	(0.0212)		
during sanction: t=1			0.1512***	0.0837***
			(0.0184)	(0.0268)
during sanction: t=2			0.1675***	0.0947***
			(0.0203)	(0.0299)
during sanction: t=3			0.2365***	0.0846**
			(0.0264)	(0.0353)
during sanction: t=4			0.1596***	-0.0229
			(0.0371)	(0.0417)
during sanction: t=5			0.2216***	-0.0086
			(0.0633)	(0.0570)
during sanction: t=6			0.1362**	0.0635
			(0.0685)	(0.0620)
during sanction: t=7			0.0076	-0.2002***
			(0.1106)	(0.0767)
during sanction: t=8			0.4458***	0.0241
			(0.0979)	(0.0813)
during sanction: t=9			0.3678***	0.3346***
			(0.1259)	(0.1240)
during sanction: t=10			0.1605	0.2725*
			(0.1597)	(0.1396)
after sanction: t=1			0.1832***	0.0953***
			(0.0189)	(0.0279)
after sanction: t=2			0.2003***	0.1127***
			(0.0192)	(0.0283)
after sanction: t=3			0.2484***	0.1709***
			(0.0187)	(0.0277)
after sanction: t=4			0.1884***	0.1692***

			(0.0194)	(0.0274)
after sanction: t=5			0.1734***	0.1435***
			(0.0189)	(0.0279)
after sanction: t=6			0.186***	0.1647***
			(0.0183)	(0.0262)
after sanction: t=7			0.186***	0.1988***
			(0.0181)	(0.0261)
Control variables	yes	yes	yes	yes
Country-pair FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
N	788,053	729,759	788,053	729,759
R-squared	0.336	0.355	0.336	0.355

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in columns (1) and (3) is the natural log of exports of country i to country *j* in year *t*, in columns (2) and (4) – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for *i* and *j*, log GDP for *i* and *j*, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Which countries does the sanction target intensify its trade relations with? The next three subsections offer answers in this regard.

5.2.1 Trade with authoritarian countries

If a country is sanctioned, it may become constrained in its trading partner choice. Being ostracized by the international community, the targeted country may turn to trade with authoritarian states that are less troubled by the issues for which the country was sanctioned. China's lack of concern for human rights and democracy led sanctioned countries such as Myanmar, Sudan, Zimbabwe and Iran to increase economic relations with China when Western countries pulled out (Pegg, 2012). If there are substitute trading partners that are motivated only by economic prospects and do not apply any criteria on democracy and transparency to their trade and investment decisions, then a sanctioned country may not need to change its behavior and the sanction may lose its bite. Moreover, if the target is able to increase engagement with (other) autocratic states, then this would undermine the sender's goals of promoting democracy and respect for human

rights, limiting arms and nuclear proliferation or increasing compliance with international law (Zweig and Jianhai, 2005).

In Table 4.5, I examine how the political regime of non-sanctioning trade partners of the target affect their trade relationship by interacting the dummies for ongoing-sanction and post-sanction periods with a measure for a country's democracy level. To quantify the political orientation of the target's trade partners, I use the Polity IV index (Marshall and Jaggers, 2013). It measures political regime type over time on a scale from -10 (autocracy) to 10 (democracy). For ease of interpretation and with regard to the practice in the political science literature, I construct a time-varying dummy $Autocratic_{jt}$ that is equal to one whenever the Polity IV index falls below zero and is zero otherwise. The interaction term between $Autocratic_{jt}$ and $During\_Nonseder_{ijt}$ and $After\_Nonsender_{ijt}$ is always negative, revealing that the positive impact of trade with non-senders during and after sanction is smaller for non-democratic trade partners of the target. This interaction effect is predicted by the empirical literature on trade and political regimes – both pairs of autocracies and mixed pairs (democracy-autocracy) are found to trade less than democratic pairs (Morrow et. al., 1998; Mansfield et. al., 2000; Aidt and Gassebner, 2010). Autocratic countries often feature weak domestic institutions that discourage trade and autocratic rulers often cater to domestic political elites with strong interest in trade protection.

TABLE 4. 5: TARGET'S TRADE WITH AUTOCRATIC NON-SANCTIONING COUNTRIES DURING AND AFTER SANCTION

	ln exports	ln imports
	(1)	(2)
ongoing sanction for i where j is not the sender	0.0814***	0.0468**
	(0.0204)	(0.0199)
autocratic	-0.1151***	0.0063
	(0.0179)	(0.0190)
ongoing sanction for i where j is not the sender # autocratic	-0.0687**	-0.0999***
	(0.0308)	(0.0367)
7-year post-sanction period for i where j was not the sender	0.1293***	0.1112***
	(0.0168)	(0.0177)
7-year post-sanction period for i where j was not the sender # autocratic	-0.0792***	-0.0306
	(0.0252)	(0.0321)
Control variables	yes	yes
Country-pair FE	yes	yes
Year FE	yes	yes
N	701517	668612
R-squared	0.350	0.380

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in column 1 is the natural log of exports of country i to country *j* in year *t*, in column 2 – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for i and j, log GDP for i and j, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

5.2.2 Trade with geopolitically aligned countries

There is evidence that countries with similar political ideologies and military allies trade more with each other (Dixon and Moon, 1993; Bliss and Russett, 1998; Long and Leeds, 2006; Nitsch, 2007; Mityakov et. al, 2013; Berger et. al., 2013). According to this line of research, the target may intensify its trade relations with countries that are ideologically sympathetic towards its regime or have common security goals.

In order to test for this hypothesis, I examine whether the target intensified its exports to and imports from countries with either an ideology similar to that of the target or with a military alliance with the target. To measure ideological

closeness between states, I use a voting similarity index ranging from 0 to 1 (increasing in countries' voting similarity), based on voting data from the UN General Assembly (Strezhnev and Voeten, 2017). Data on military alliances comes from the Correlates of War Formal Alliance dataset (Gibler, 2008). I use a dichotomous variable coded one to signify the existence of a defense pact between two countries.

Armed with these two measures of geopolitical closeness, I introduce them in my gravity model by interacting them in separate regressions with the dummies $During\_Nonseder_{ijt}$ and $After\_Nonsender_{ijt}$. Tables 4.6 and 4.7 provide the estimation results. In all cases, the coefficient of the interaction with voting similarity is negative and statistically significant. The effect of increased trade with non-senders decreases with proximity in international politics. The interactions with military alliance are not statistically significant, implying that trade intensification between the target and non-sanctioning countries does not depend on close military ties. These results signal that in the years during and after sanction the target does not engage in more trade with geopolitically close countries, relative to other years. Yet, the overall effect of UN voting similarity and military alliances is positive and statistically significant. This is in line with the findings of the literature on the positive impact of political proximity between countries and bilateral trade. On the other hand, target countries are often sanctioned because of internal or external political instability, such as civil wars, territorial disputes, support of terrorism or major human rights violations. These types of events represent an additional transaction cost that reduces bilateral trade around the time of the event, and the sanction, respectively (Blomberg and Hess, 2006; Moser et. al., 2008; Martin et. al., 2008a; Martin et. al., 2008b; Glick and Taylor, 2010).

TABLE 4. 6: TARGET'S TRADE WITH NON-SANCTIONING GEOPOLITICAL ALLIES OF THE TARGET DURING AND AFTER SANCTION

	ln exports	ln imports
	(1)	(2)
ongoing sanction for i where j is not the sender	0.2614***	0.3047***
	(0.0540)	(0.0684)
UN voting similarity with target	0.4884***	0.4649***
	(0.0475)	(0.0482)
ongoing sanction for i where j is not the sender # UN voting similarity with target	-0.2455***	-0.3651***
	(0.0675)	(0.0826)
7-year post-sanction period for i where j was not the sender	0.2274***	0.2633***
	(0.0450)	(0.0659)
7-year post-sanction period for i where j was not the sender # UN voting similarity with target	-0.1642***	-0.2144***
	(0.0550)	(0.0773)
Control variables	yes	yes
Country-pair FE	yes	yes
Year FE	yes	yes
N	675,033	628,070
R-squared	0.312	0.332

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in column 1 is the natural log of exports of country i to country *j* in year *t*, in column 2 – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for i and j, log GDP for i and j, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

TABLE 4. 7: TARGET'S TRADE WITH NON-SANCTIONING MILITARY ALLIES OF THE TARGET DURING AND AFTER SANCTION

	ln exports	ln imports
	(1)	(2)
ongoing sanction for i where j is not the sender	0.0612***	0.0003
	(0.0161)	(0.0194)
military alliance with target	0.0691**	0.0441
	(0.0324)	(0.0318)
ongoing sanction for i where j is not the sender=1 # military alliance with target	-0.0549	0.0267
	(0.0506)	(0.0502)
7-year post-sanction period for i where j was not the sender	0.1022***	0.0902***
	(0.0133)	(0.0170)
7-year post-sanction period for i where j was not the sender # military alliance with target	0.0297	0.0428
	(0.0376)	(0.0388)
Control variables	yes	yes
Country-pair FE	yes	yes
Year FE	yes	yes
N	788,053	729,759
R-squared	0.336	0.354

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in column 1 is the natural log of exports of country i to country *j* in year *t*, in column 2 – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for i and j, log GDP for i and j, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

Do sender's military or political allies support the sender's stance on sanctions and reduce trade with the target? It is possible that the increase in exports and imports with non-sanctioning countries is only due to those non-senders that are geopolitically detached from the sender. Hence, the target's increase in trade that we observe is not because of stronger trade engagement with its own allies but coming from trade intensification with the geopolitical foes of the sender. To test for this possibility, I employ in separate regressions the UN voting similarity index and the military alliance indicator, but this time between sender and non-sanctioning countries and interact those measures with the dummies $During\_Nonseder_{ijt}$ and $After\_Nonsender_{ijt}$. Tables 4.8 and 4.9 report the results. The coefficients on the interactions of the two geopolitical measures with $During\_Nonseder_{ijt}$ are not statistically significant. However, the interactions

with $After\_Nonsender_{ijt}$ are statistically significant and positive for exports. Thus, I find evidence for differential impact in the post-sanction period based on a non-sender country's political closeness to the sender. The target exports more strongly to those countries that are aligned, politically or militarily, with the sender. I also find that the differential impact of political alignment is negative for imports. For one of the measures of geopolitical proximity – voting similarity in the UN – the sign on the interaction coefficient is negative and statistically significant. However, this is weaker evidence as the other measure, military alliance, turns out to be not statistically significant.

This opportunistic type of behavior implies that countries that are geopolitically close to the sender see the imposition of sanction as a chance to strengthen trade with the target during a period of diminished or no competition. Sanctions may present an opportunity for non-senders to enter or expand in markets previously dominated by the sanction sender (McLean and Whang, 2010).

TABLE 4. 8: TARGET'S TRADE WITH NON-SANCTIONING GEOPOLITICAL ALLIES OF THE SENDER DURING AND AFTER SANCTION

	ln exports	ln imports
	(1)	(2)
ongoing sanction for i where j is not the sender	-0.0400	0.1049
	(0.0440)	(0.0915)
UN voting similarity with sender	-0.146*	0.4584***
	(0.0837)	(0.1412)
ongoing sanction for i where j is not the sender # UN voting similarity with sender	0.0754	-0.1564
	(0.0597)	(0.1112)
7-year post-sanction period for i where j was not the sender	-0.1922***	0.1835*
	(0.0564)	(0.1034)
7-year post-sanction period for i where j was not the sender # UN voting similarity with sender	0.2173***	-0.2373*
	(0.0750)	(0.1252)
Control variables	yes	yes
Country-pair FE	yes	yes
Year FE	yes	yes
N	94,588	80,203
R-squared	0.312	0.222

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in column 1 is the natural log of exports of country i to country *j* in year *t*, in column 2 – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for i and j, log GDP for i and j, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

TABLE 4. 9: TARGET'S TRADE WITH NON-SANCTIONING MILITARY ALLIES OF THE SENDER DURING AND AFTER SANCTION

	ln exports	ln imports
	(1)	(2)
ongoing sanction for i where j is not the sender	0.0573***	0.0204
	(0.0177)	(0.0215)
military alliance with sender	-0.0559	-0.0374
	(0.0367)	(0.0333)
ongoing sanction for i where j is not the sender # military alliance with sender	0.0347	-0.0254
	(0.0413)	(0.0440)
7-year post-sanction period for i where j was not the sender	0.0919***	0.1043***
	(0.0140)	(0.0183)
7-year post-sanction period for i where j was not the sender # military alliance with sender	0.1071***	0.0003
	(0.0396)	(0.0416)
Control variables	yes	yes
Country-pair FE	yes	yes
Year FE	yes	yes
N	788,053	729,759
R-squared	0.336	0.354

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in column 1 is the natural log of exports of country i to country *j* in year *t*, in column 2 – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for i and j, log GDP for i and j, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

5.2.3 Trade with export competitors of the sender

When the US imposed sanctions on Turkey after its invasion of Northern Cyprus in 1974, Turkey intensified its trade relations with its other major trading partners – France, Germany, Italy and the UK (Navarro, 1978). A US-imposed embargo on Iran and Iraq during the Iran-Iraq war in the 1980s diverted the arms trade of the two sanction targets towards France and Italy (Kamel, 2016). After the coup in Gambia in 1994, sugar exporters France, Germany and Netherlands restricted their trade with the country. In response, Gambia increased its sugar imports from another sugar exporter, Belgium. A sanction target looks for close substitutes with respect to the sanction sender's trade flows. How common is

trade deviation towards countries with similar economic structure as the sanction sender?

To test this idea, I explore non-senders' export similarity with the sanctioning country. In particular, I measure the overlap in two countries' export baskets – the sanction sender and the non-sender – employing the export similarity index (Finger and Kreinin, 1979). The index is the sum of the two countries' minimum share in each good for every period:

$$ESI_t^{S,NS} = \sum_p \min(s_{p,t}^S, s_{p,t}^{NS})$$

where $s_{p,t}^S$ is the share of the sender country's export value in product *p* relative to all its exports, in year *t*. The index varies between 0 – countries do not have any exports in common – and 1 – the two countries have identical exports structure. For the index construction I use data at the two-digit SITC level (product-group level data) which comes from UN COMTRADE, with corrections made by Hausmann et al. (2011) and made available by the Atlas for Economic Complexity project. I use the export similarity index in its continuous form and introduce it in my gravity equation (4.2) by interacting it with the dummies $During\_Nonseder_{ijt}$ and $After\_Nonsender_{ijt}$. In other words, I allow the effect of trade with nonsender during and seven years after sanction to differ across countries that are more or less similar in their exports to the sanction sender.

The coefficients on the interaction terms are both positive and statistically significant (Table 4.10) for the imports of the target. This implies that the sanction target reacts with trade diversion to other trading partners that offer similar export goods. The effect of intensified trade with competitors of the sanction sender continues also after the sanction is lifted. Intuitively, the target turns to trade with countries that are likely to be the next best alternative markets.

TABLE 4. 10: TARGET'S TRADE WITH NON-SANCTIONING COUNTRIES WITH EXPORT STRUCTURE SIMILAR TO THE ONE OF THE SENDER, DURING AND AFTER SANCTION

	ln exports	ln imports
	(1)	(2)
ongoing sanction for i where j is not the sender	-0.0048	-0.0198
	(0.0218)	(0.0347)
export similarity with sender	0.8133	-2.2124**
	(0.8785)	(0.9722)
ongoing sanction for i where j is not the sender # export similarity with sender	0.7042	2.2591***
	(0.7645)	(0.8402)
7-year post-sanction period for i where j was not the sender	-0.0780***	-0.0162
	(0.0264)	(0.0384)
7-year post-sanction period for i where j was not the sender # export similarity with sender	0.2102	1.6412*
	(0.8013)	(0.9191)
Control variables	yes	yes
Country-pair FE	yes	yes
Year FE	yes	yes
N	80525	66475
R-squared	0.232	0.179

Notes: The unit of observation is a country-pair in year *t*, where *t* ranges from 1948 to 2015. The dependent variable in column 1 is the natural log of exports of country i to country *j* in year *t*, in column 2 – natural log of imports of country *i* to country *j* in year *t*. Control variables include log population for i and j, log GDP for i and j, indicator for both countries in GATT/WTO, indicator for regional trade agreement, indicator for common currency. Standard errors are clustered at the country-pair level.
* p < 0.10, ** p < 0.05, *** p < 0.01

These results speak to a set of results on trade diversion to countries offering similar products, in the case of antidumping measures. In particular, numerous studies (Prusa, 2001; Konings et. al. 2001; Niels, 2003; Durling and Prusa, 2006; Bown and Crowley, 2007; Vandenbussche and Zanardi, 2010) have found that starting an antidumping case against another country decreases bilateral exports to that country to the benefit of the increased trade of the antidumping defendant with other trading partners. Additionally, the initiator of the antidumping case also diverts its trade flows towards third parties. However, this deviation does not fully account for the lost exports with the antidumping case defendant and overall, the net exports of the country imposing antidumping measures decline.

6. Conclusion

I find evidence that sanction imposition decreases bilateral trade between the sender and the target but does not depress target's total trade. The target diverts its trade flows towards non-sanctioning countries. My estimates across the different non-sanctioning countries vary substantially, showing that economic motives and not political alliances lead to the trade intensification between a target and non-senders in the years during and after sanction. My results have implications for the empirical research on the impact of (trade-based) sanctions, trade disputes and trade wars. I show that a country's sanction policy has a persistent impact on the target's trade behavior. This is especially significant when the target is an important trading partner of the sender.

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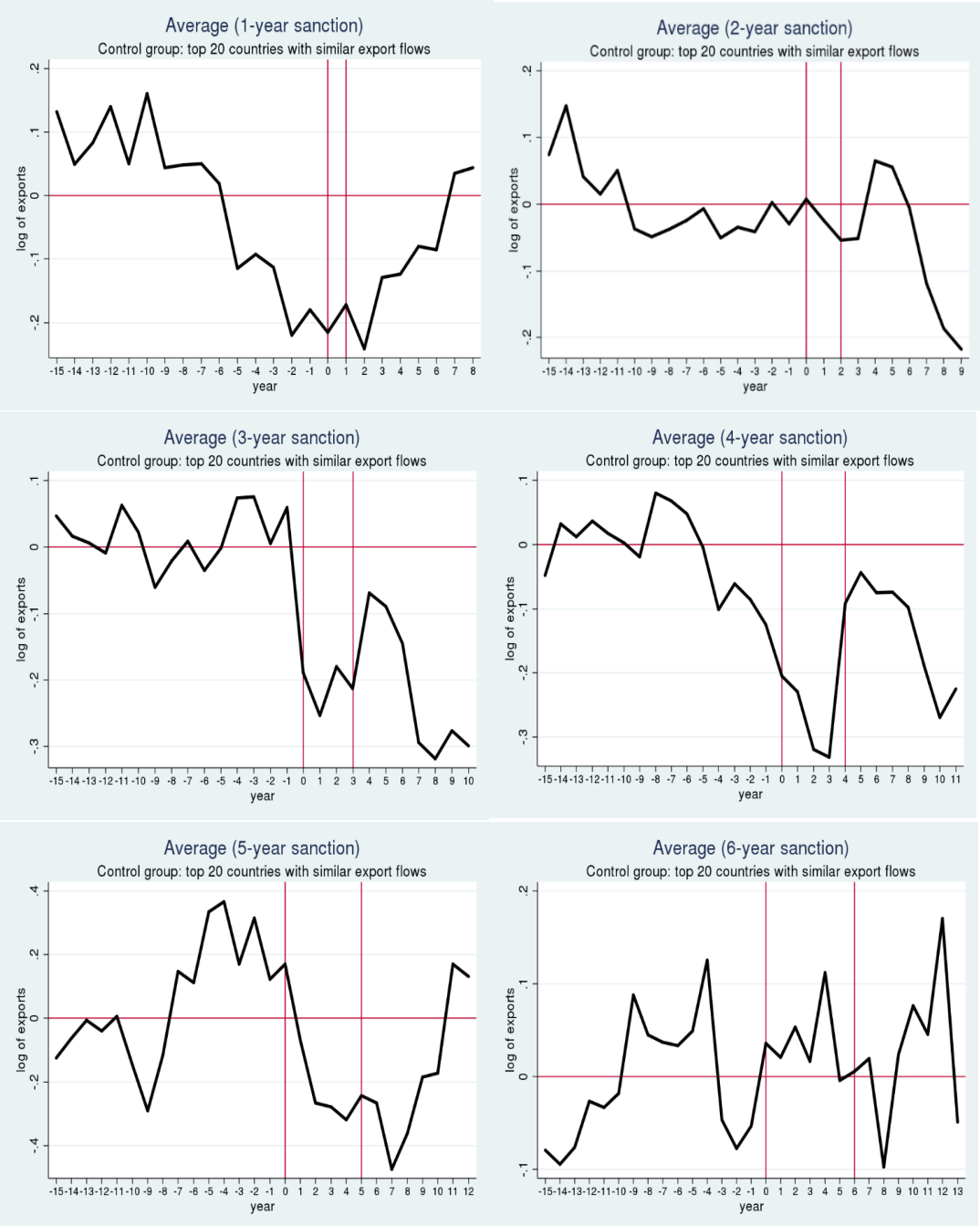
APPENDIX A4

FIGURE A4. 1: TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

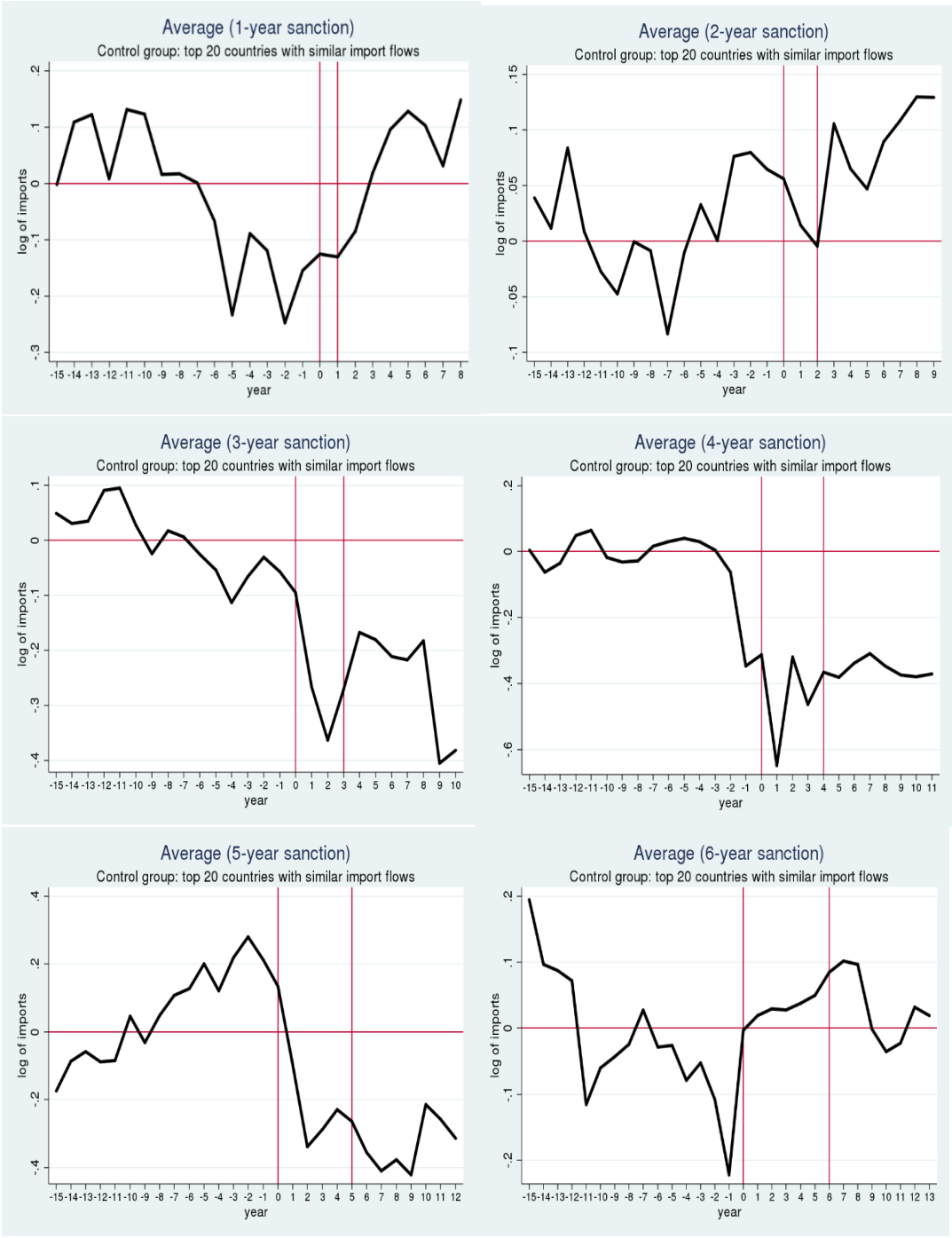


Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. Only sanction cases with pre-treatment RMSPE < 0.4 are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE A4. 1(CONTINUED): TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

Notes: Figure continued from previous page

FIGURE A4. 2: TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

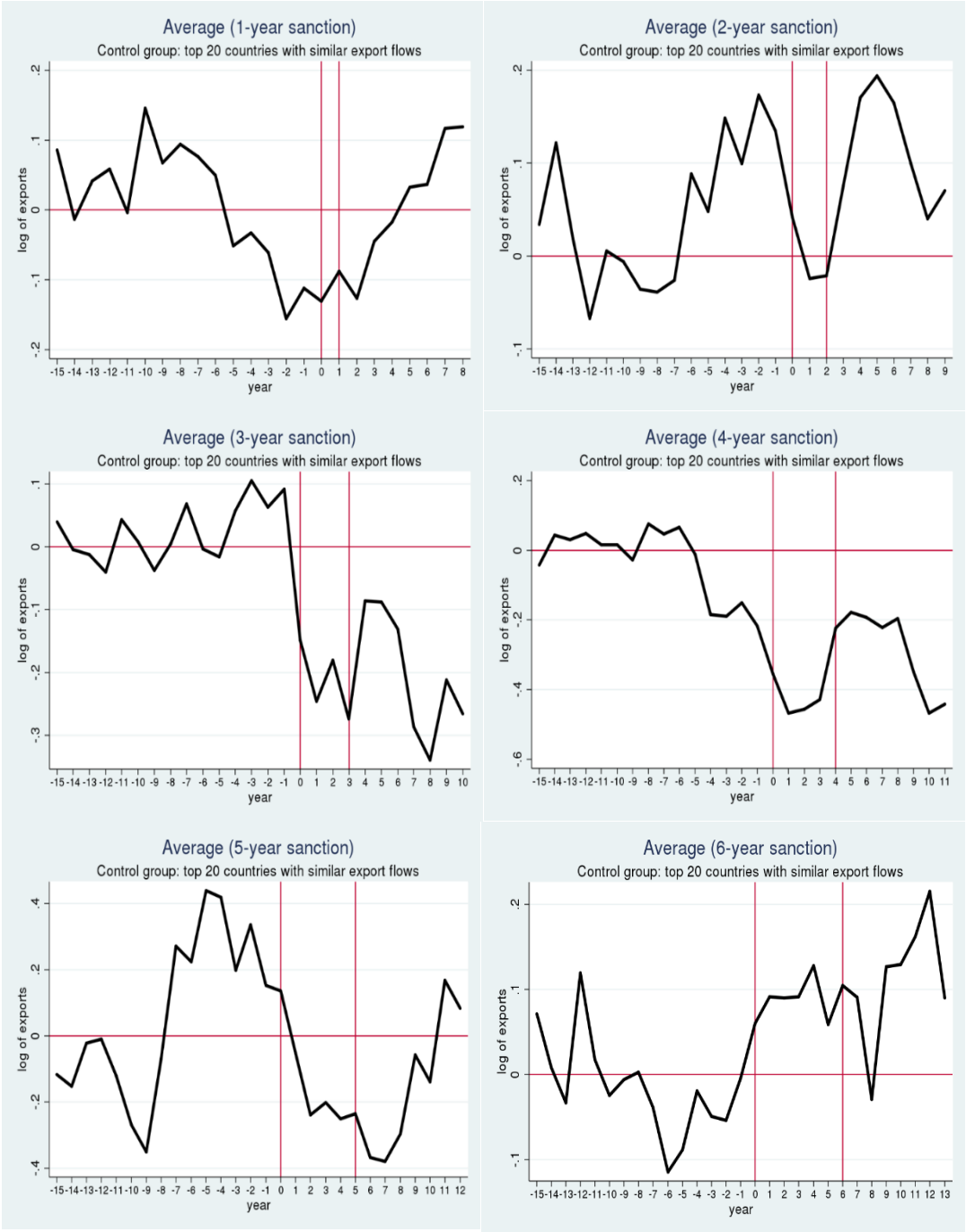


Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. Only sanction cases with pre-treatment RMSPE < 0.4 are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE A4. 2 (CONTINUED): TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

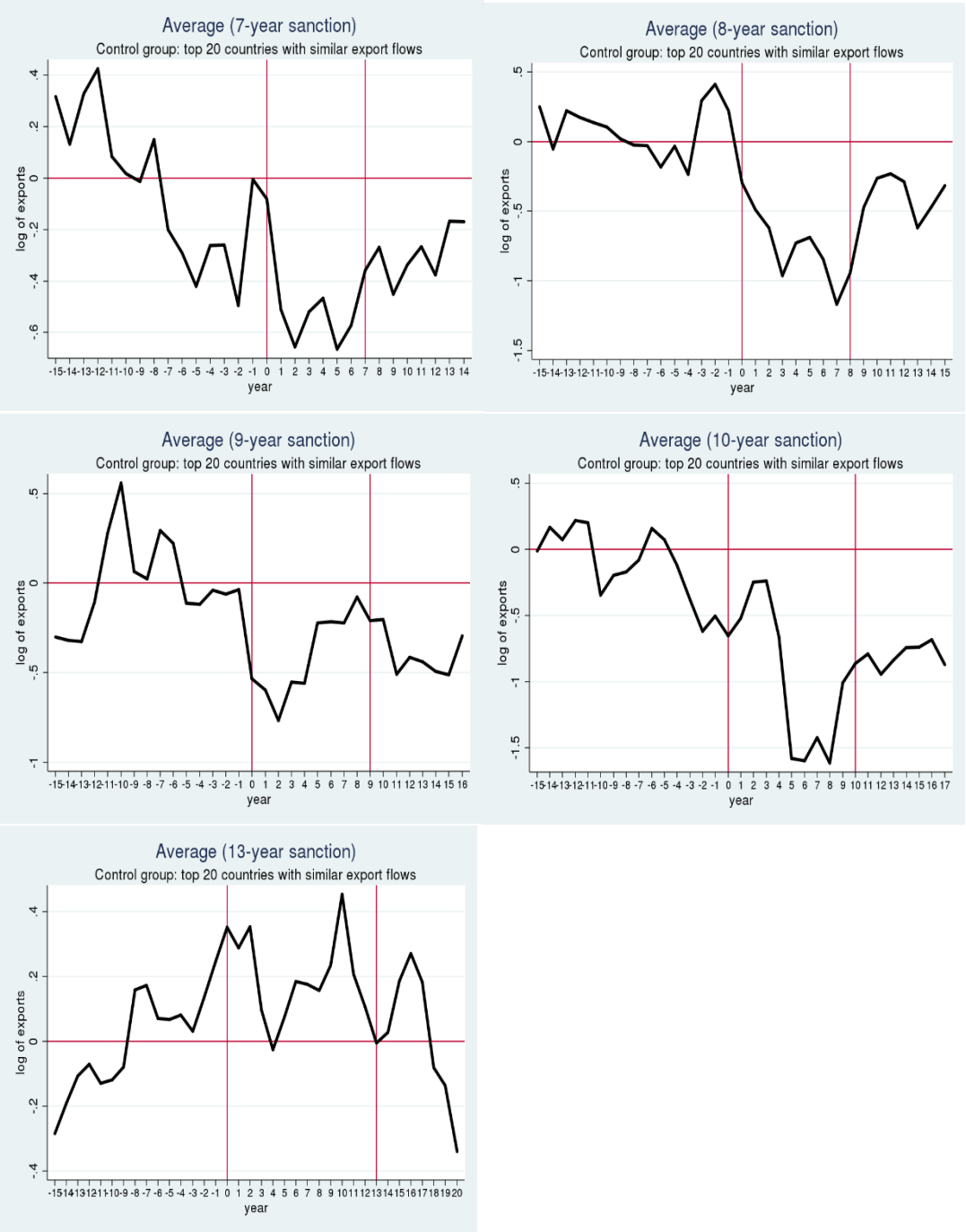
Notes: Figure continued from previous page

FIGURE A4. 3: TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. Only sanction cases with pre-treatment RMSPE < 0.8 are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE A4. 3 (CONTINUED): TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



Notes: Figure continued from previous page

FIGURE A4. 4: TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. Only sanction cases with pre-treatment RMSPE < 0.8 are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

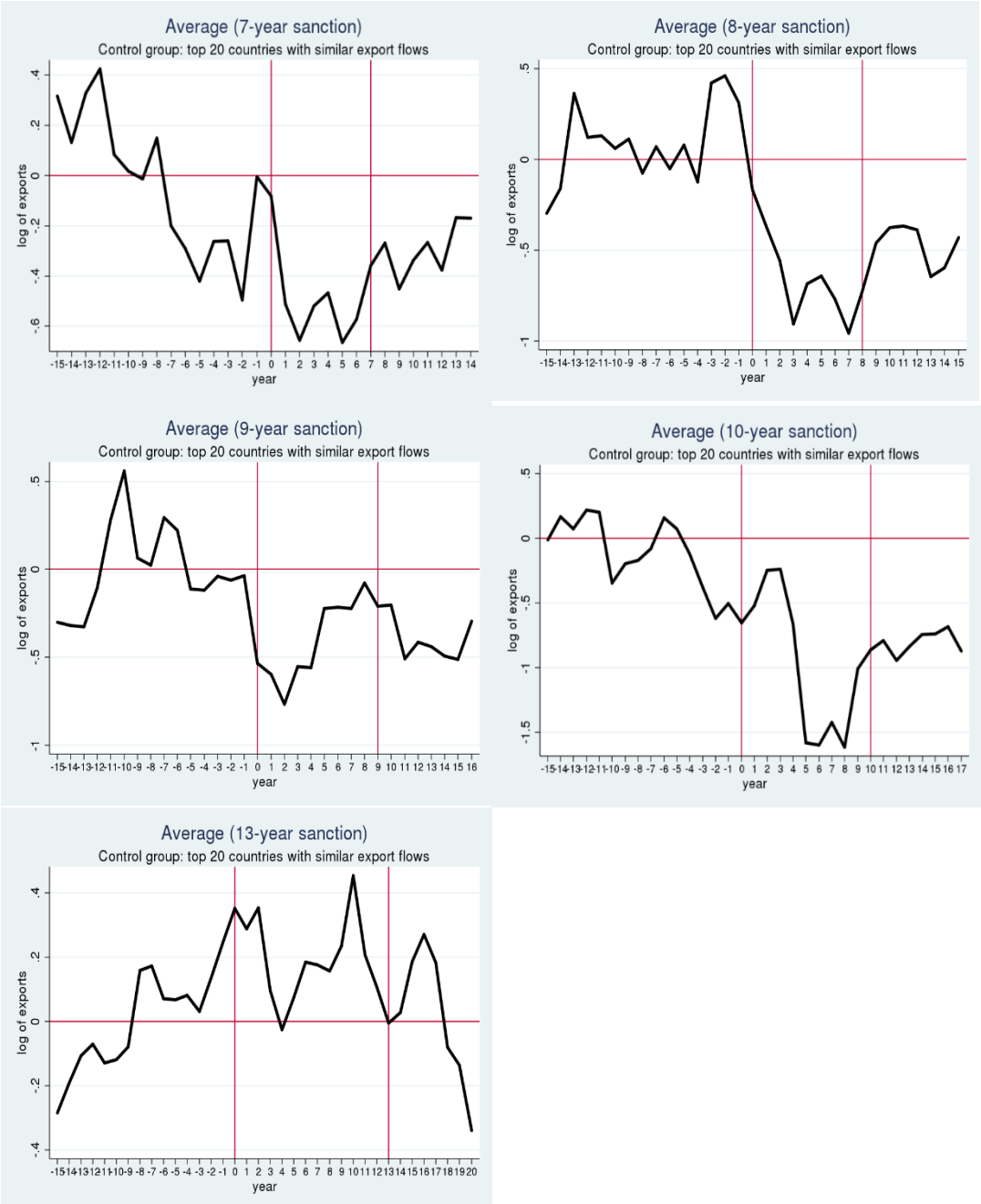
FIGURE A4. 4 (CONTINUED): TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

Notes: Figure continued from previous page

FIGURE A4. 5: TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. All sanction cases regardless of pre-treatment fit are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE A4. 5 (CONTINUED): TREND OF AVERAGE LOG EXPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

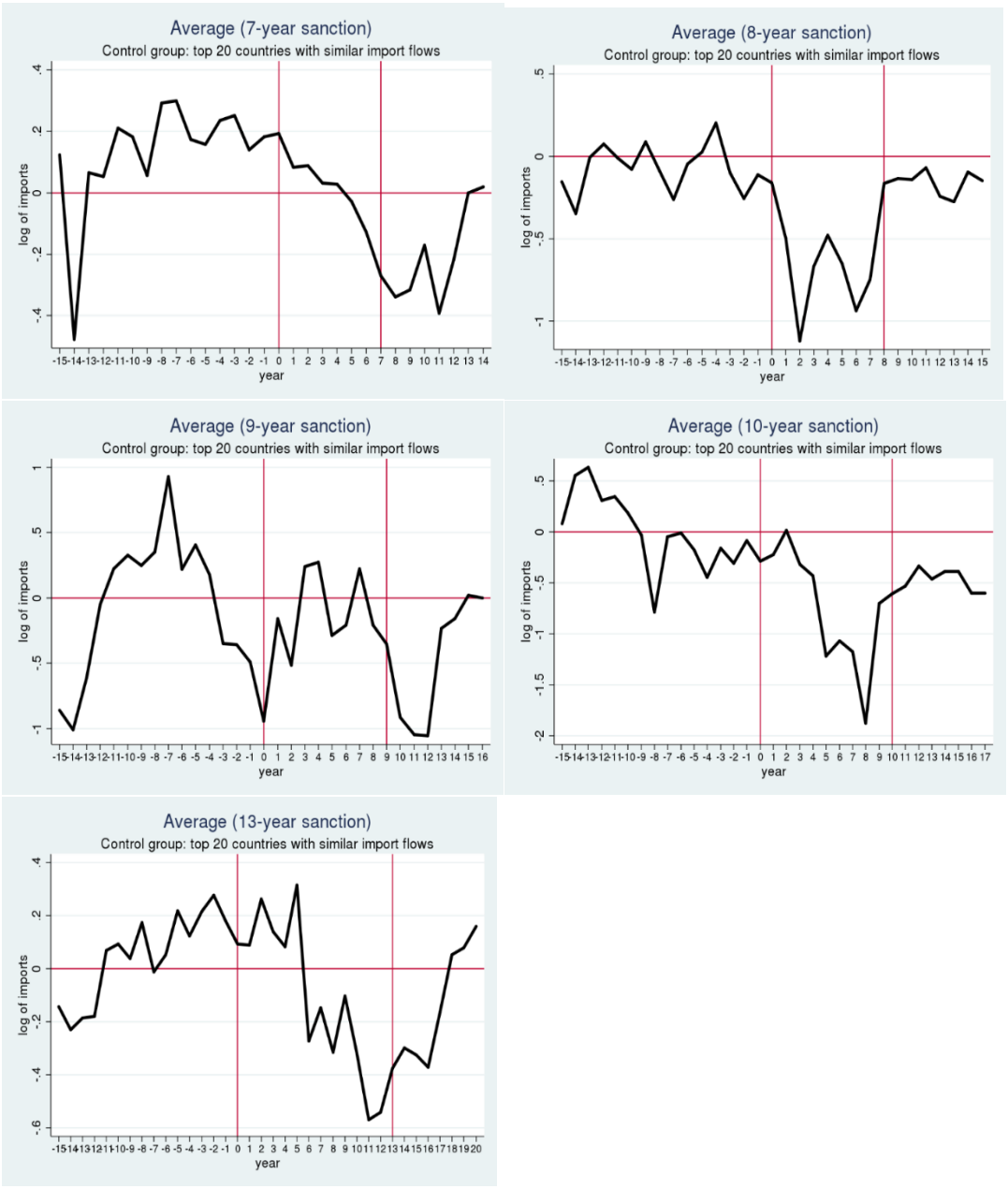


Notes: Figure continued from previous page

FIGURE A4. 6: TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD

Notes: These figures plot the difference between sanctioned countries and a synthetic control country consisting of similar countries that did not contemporaneously experience a sanction. Sanction cases are grouped according to sanction length. All sanction cases regardless of pre-treatment fit are included. Pre-intervention matching until year -4. The first red vertical line denotes the initiation of a sanction, the second one – the end of a sanction.

FIGURE A4. 6 (CONTINUED): TREND OF AVERAGE LOG IMPORTS GAP WITH THE SYNTHETIC CONTROL METHOD



Notes: Figure continued from previous page

TABLE A4. 1: LIST OF SANCTION CASES

Sender	Target	Start Year	End Year
United States	Ecuador	1965	1977
United States	Peru	1965	1977
France	Israel	1967	1974
United Kingdom	Israel	1967	1967
United States	Italy	1968	1971
United States	Japan	1969	1972
United States	Australia	1973	1976
Libya	Netherlands	1973	1974
United Kingdom	Chile	1974	1980
United States	India	1974	1976
Canada	India	1974	1976
United States	Taiwan Province of China	1976	1977
United States	Guatemala	1977	1983
Canada	Japan	1977	1978
United States	Pakistan	1977	1977
United States	Iran, Islamic Republic of	1979	1981
Japan	Iran, Islamic Republic of	1979	1981
United Kingdom	Iran, Islamic Republic of	1979	1981
Italy	Iran, Islamic Republic of	1979	1981
France	Iran, Islamic Republic of	1979	1981
France	Poland	1980	1984
United States	Poland	1980	1984
Belgium	Poland	1980	1984
United Kingdom	Poland	1980	1984
Germany	Poland	1980	1984
Belgium	Russia	1980	1982
United Kingdom	Russia	1980	1982
United States	Israel	1981	1981
France	Italy	1981	1982
United States	Nicaragua	1981	1990
Germany	Turkey	1981	1986
United Kingdom	Turkey	1981	1986
France	Turkey	1981	1986
United Kingdom	Argentina	1982	1989
Germany	Argentina	1982	1989
Japan	Argentina	1982	1989
United States	Argentina	1982	1989
Germany	Canada	1982	1984
Italy	Canada	1982	1984
Belgium	Canada	1982	1984
United Kingdom	Canada	1982	1984

France	Canada	1982	1984
United States	Germany	1982	1982
Italy	Spain	1982	1984
Belgium	Spain	1982	1984
France	Spain	1982	1984
United Kingdom	Spain	1982	1984
Germany	Spain	1982	1984
Turkey	France	1982	1982
United States	United Kingdom	1982	1982
Germany	Israel	1982	1983
Belgium	United States	1983	1984
France	United States	1983	1984
Italy	United States	1983	1984
Germany	United States	1983	1984
United States	New Zealand	1985	1990
Canada	Germany	1986	1987
Canada	France	1986	1987
Canada	Greece	1986	1987
Canada	Netherlands	1986	1987
Canada	Portugal	1986	1987
United Kingdom	Syrian Arab Republic	1986	1994
Germany	Syrian Arab Republic	1986	1994
United States	Syrian Arab Republic	1986	1994
Japan	China,P.R.: Mainland	1987	1988
United States	Panama	1988	1997
United States	Venezuela, Rep. Bol.	1988	1997
Germany	China,P.R.: Mainland	1989	1990
France	China,P.R.: Mainland	1989	1990
United Kingdom	China,P.R.: Mainland	1989	1990
Germany	Iran, Islamic Republic of	1989	1998
Sweden	Iran, Islamic Republic of	1989	1998
Germany	Japan	1989	1990
France	Haiti	1991	1994
United States	Haiti	1991	1994
Canada	Haiti	1991	1994
Switzerland	Turkey	1991	1992
United States	Colombia	1992	1997
United States	Costa Rica	1992	1997
United States	Spain	1992	1997
United States	Indonesia	1992	1992
Canada	Australia	1993	1995
Canada	Nigeria	1993	1999
United States	Nigeria	1993	1999
United Kingdom	Nigeria	1993	1999

Canada	New Zealand	1993	1993
Australia	United States	1993	1993
Germany	Korea, Rep.	1994	1996
New Zealand	France	1995	1996
Japan	France	1995	1996
Australia	France	1995	1996
United States	Lebanon	1995	2000
United States	Turkey	1995	1998
Democratic Republic of the Congo	Burundi	1996	1999
Sweden	United Kingdom	1996	1999
Germany	United Kingdom	1996	1999
France	United Kingdom	1996	1999
Netherlands	United Kingdom	1996	1999
Belgium	United Kingdom	1996	1999
Indonesia	Australia	1997	1997
France	Iran, Islamic Republic of	1997	1997
Colombia	Japan	1998	1998
Australia	Pakistan	1998	2001
Germany	Pakistan	1998	2001
Canada	Pakistan	1998	2001
United States	Pakistan	1998	2001
Japan	Pakistan	1998	2001
United States	Australia	1999	2001
United Kingdom	Ethiopia	1999	2000
United States	Ethiopia	1999	2000
Indonesia	China,P.R.: Mainland	2000	2000
Australia	Fiji	2000	2001
United Kingdom	Fiji	2000	2001
New Zealand	Fiji	2000	2001
Canada	India	2000	2005
Indonesia	Korea, Rep.	2000	2000
United Kingdom	Liberia	2000	2003
Indonesia	Singapore	2000	2000
India	Pakistan	2001	2003
Colombia	Chile	2002	2006
China,P.R.: Mainland	United States	2002	2003
Japan	Canada	2003	2005
United States	Israel	2004	2005
Syrian Arab Republic	Lebanon	2005	2005
Thailand	Malaysia	2005	2007

Selbstständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbstständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.
Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den 03. September 2018 Aleksandra Peeva